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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Some Remarks on the History and Origin of Syphilis.*
By J. L. MILTON, Surgeon to St John's Hospital, London.

IT has often surprised me that in this country so little attention should have been given to the history of syphilis. To me it seems one of the greatest problems in the records of medicine; for, either a new disease sprang up at the close of the fifteenth century, and in time became so welded with the old one that we can no longer separate them; or an old malady, till then always local, suddenly became constitutional, and has remained so for nearly four centuries. I think I may safely ask if the annals of any disease show evidence of such a portentous change as this.

I see no way out of the dilemma in which this statement involves us. Certainly the evidence that venereal diseases of the genital organs existed from very remote times—that we can trace these back through the dark ages—is, to my mind, irrefutable. I know that a great deal of the evidence by which some authors have sought to establish this position is so obscure as to have thrown discredit on it. But I pass that by. I leave on one side such accounts as those given by Herodotus of the disease under which the Scythians suffered, which was as likely to have been leprosy as anything else;¹ that to which Horace alludes as attacking Cleopatra and her troop; the warts and condylomata spoken of

¹ Clio, 105. At any rate, it was not gonorrhœa, as has been surmised; for the old historian says, those who come into the country of the Scythians may see, *ὁπῶρ*, in what manner those persons are afflicted.

by Juvenal; and many passages in the old authors, from the close of the twelfth to near the end of the fifteenth century. The reader will find these very carefully analyzed by M. Robert,¹ Simon of Hamburg,² and others. I proceed to a few trenchant instances, which, taken in connexion, seem, in my judgment, too strong to be withstood.

If the ulcers on the glans and foreskin,—sometimes dry, with very little secretion, sometimes phagedænic,—the rhagades and condylomata, the hard, almost insensible excrescences described by Celsus, the buboes and destruction of the penis by phagedæna, sketched on the votive tablets which were found in the ruins of the old temple near the Seine, and described by Becquerel,³ were not syphilis, what were they, and what interpretation would be put on such symptoms in our day? What else was the boil which the hermit Ero, as told by Palladius, got from an actress, and which destroyed great part of the holy father's glans penis? What was the sloughing phagedæna for which Avicenna suggested amputation of the penis?

Gordon and John of Gaddesden speak of sores piercing between the skin and flesh (*puncturæ inter corium et carnem*) of the foreskin. John of Gaunt, we are told, died in 1399, and King Ladislaus died at Naples in 1414, of phagedænic disease of the genital organs, complaints which are usually ascribed in the nineteenth century to a venereal origin. Thomas Gascoigne, quoted by Simon, and writing in the first half of the fifteenth century, speaks of several men of distinction who had suffered from sloughing of these parts. The famous traveller Dr Clarke, in his account⁴ of a manuscript in the library at Stockholm, by Johannes Arderum de Sleewark, which must have been written rather early in the fifteenth century, inasmuch as it was the work of a man who practised at Newark from 1412 to 1419, says,—“It is very curious to observe (in this manuscript) ‘Pro morbo qui dicitur’ . . . followed by the French name of a disorder which is supposed not to have been known in Europe before the discovery of America.” It is very curious indeed; for, if this be not a later interpolation, there is an end of the question.

As we approach the era of the siege of Naples, the evidence grows more decisive. That abandoned scamp, Villon, who, according to Simon, passed half his life in brothels, speaks of chancres and condylomata (*fics*) as if they were quite common affairs in his day. Certainly, he was likely to be a pretty good authority on the matter. Peter Martyr, in a letter to the great Portuguese scholar Barbosa, dated 5th April 1488, mentions the disease in the most unequivocal manner. The correctness of the

¹ Nouveau Traité des Maladies Veneriennes. Par le Docteur Melchior Robert. 1861.

² Ricord's Lehre von der Syphilis. Von Dr F. A. Simon. 1851.

³ Union Médicale, 22d December 1852.

⁴ Travels, by E. D. Clarke. Vol. xi. p. 139. 1824.

date has been disputed by Thiene,¹ who, however, wrote at a time when men believed as firmly in the sophistical reasoning of Astruc as they did in the circulation of the blood; but I think Mr Prescott has quite disposed of his objections.² Vella was acquainted with primary symptoms. He saw the disease both before and after the outbreak of the morbus Gallicus, and expresses his surprise at finding it from this date followed by secondary affections.³ Many of the physicians who, at the close of the fifteenth century, saw the disorder for the first time, so far from looking upon it as a malady hitherto unknown, thought it was a return of the ancient leprosy, with the descriptions of which, we are told, those of them who had read the works of the Arabian writers were quite familiar.⁴ John de Vigo even fancied that he discovered in syphilis the malady under which the Emperor Augustus suffered,—an extraordinary error for so able an observer, seeing that there is not the least resemblance between the two.

But there must be some mistake here, some confusion which requires to be cleared up. I know of but two distinct affections to which the name of leprosy has ever been generally given: lepra, including under this head the psoriasis of Willan; and elephantiasis Græcorum, which we now consider to be true leprosy. It is not very probable that men would ever confound the former with syphilis; and if the latter had really died out previously, to come back at this time with renovated malignity, and infest Europe for centuries after, I believe I am correct in saying that such a fact, if fact it be, is quite unknown. But there is not a tittle of evidence to show that the real leprosy had even materially declined at the close of the fifteenth century, still less that it had disappeared. With this awful disease so near at hand,—a disease which a man who has never seen it will recognise from an account read years ago, which, once seen, no man can ever forget,—how is it possible they could confound syphilis at one and the same time with it and the “leprosy of the ancients”—elephantiasis Græcorum? The disease really answering to this description was at their very doors, and its long, unbroken descent down to their day must have been matter of notoriety.

The recognition, then, of the morbus Gallicus as the leprosy of the older writers, proves, assuming it to be founded on truthful and trustworthy observation, not a similarity between syphilis and elephantiasis Græcorum, but between the former and some disease as yet not made out. What this was, is perhaps destined to remain an enigma. Most probably the disease which they took for leprosy never existed at all. The later Greek and Arabian writers

¹ Lettere sulla Storia dei Mali Venerei, Venezia, 1823. Quoted by Prescott.

² History of Ferdinand and Isabella. By William H. Prescott. Vol. ii. p. 202. 1851.

³ Ricord's Lehre, p. 7.

⁴ Under the names of bothor, saphati (a papulo-squamous eruption), and formica, which answers pretty well to our idea of lupus.

had jumbled together under this name eczema, lepra, elephantiasis, leprosy itself, symptoms which seem to belong to syphilis, and, possibly, scabies. The Italian surgeons might, therefore, very well be excused for taking up only a confused idea of such a compound, the true nature of which was really not revealed till the labours of Mason Good shed light upon the scene; and if they saw in syphilis the leprosy of older times, and not the existing leprosy, this only proves that they had before their imaginations a monster, which, as I have said, never existed but in imagination.

But there can be no doubt that the surgeons in Italy saw constitutional syphilis—at least so as to understand clearly what it was—for the first time in December 1494, when Charles VIII. passed the Alps with his army on the way to Naples. The work of John de Vigo would, I think, alone settle the point. It is impossible such a book could have been written by any but a thoroughly skilled and shrewd observer, perfectly familiar with the disease. The description of chancres, the exact knowledge of the periods of incubation, the tracing of a crowd of symptoms to their true source, the recognition of the protean nature of these symptoms, the graphic accounts of the horrible pains in the bones,—all attest the hand of a master; and it only remains to conclude, that constitutional disease, following directly an indurated sore, was a new disease to the surgeons of that day.

What, then, was the old disease? What were these sores of phagedænic nature which were never followed by secondary symptoms, as happens now with phagedæna in some instances—of sores, almost dry, which now infect the constitution? For my part, I know of no symptoms, except of syphilitic origin, which correspond to such descriptions. Astruc's reply to the question would have been, "Anything the reader likes, so long as he admits that they are not syphilitic." But how is it possible to admit this, if we are to believe that the dry and the purulent sore, the phagedænic and sloughing, the hard and soft, are but varieties of one common type, from which they all spring, and to which they all return? The dualists get out of the difficulty easily enough. True syphilis, with its long train of constitutional symptoms, is, according to them, quite distinct from the disease which was known prior to the siege of Naples; the latter was simply chancre; the two have nothing to do with each other, any more than gonorrhœa has to do with either of them. But how do those who believe in the unity of the virus propose to explain such a change as that of a local disease being transmuted into a constitutional one? The old disease, with its various forms of sore, appears on the stage much as it did in the days of Celsus; but it is now followed by secondary disease, it is accompanied by indurated chancre, and the entire aspect of syphilis has assumed, apparently for the first time, the features it has ever since worn.

Taking into account the mention of the disease under the name by which it was so long known, as far back as the early part of the

fifteenth century, the letter of Peter Martyr, and the statement by Dr Mason Good,¹ that, even in the spring of 1493, the dreaded malady was rising in Auvergne and Lombardy, I think there is good reason for thinking that the appearance of syphilis in Europe must be antedated; but that, owing to some cause of which we are totally ignorant, possibly some climatic change, like those which usher in boils, cholera, influenza, and so on, it suddenly spread about the end of the fifteenth century with great rapidity. De Vigo says that it appeared in December 1494 throughout almost all Italy; but here I think he is in error, as it seems to have found its way into Italy nearly, if not quite, two years previously. It may have been lurking in more than one part of Europe under some different and now forgotten name, just as it did more recently in Scotland and Ireland, under the names of sibbens and button scurvy.

Stress has been laid upon the fact that the earlier writers did not ascribe the disease we now believe to be simple syphilis to its true source—impure connexion; hence, that it did not arise from such a source. The inference seems to me incorrect. When we reflect how difficult it often is now, when so much has been done towards elucidating the pathology of syphilis, to say whether a sore is really due to a suspicious connexion or not, we can readily understand how much more difficult it must have been for the practitioners of the fourteenth and fifteenth centuries to trace a connexion between the two events. The fact, so long doubted but now fairly admitted, that secondary syphilis can be transmitted without any connexion at all, may have still further contributed to strengthen the error. As to the disease having been all at once noticed in so many different places, and by different persons, I do not ascribe so much weight to the fact as some persons might. It is only what is always happening. A man of unusual penetration observes the bond between cause and effect; he points it out; and when the evil passions which discovery is sure to call forth have subsided, others follow in the track which he has made for them.

Take, for instance, the history of the morbus Addisonii as an illustration in support of this statement. There is, I believe, no proof that any observer before Addison really had a clue to the tie between certain forms of bronzing and diseases of the supra-renal capsules. Yet, unless we assume that the disease sprang up *de novo* at the very time he announced it to the world, these phenomena must have yearly passed unnoticed, and certainly unsolved, before the eyes of hundreds of observers. The clue once found, any tyro can connect the two sets of symptoms.

Take, again, as an illustration, the slow discovery of many undoubted forms of syphilis, or rather their re-discovery in some cases—sarcocoele and iritis (doubted even by such an observer as John Hunter); the venereal nature of infantile syphilis; chancre set

¹ The Study of Medicine. By John Mason Good. Vol. iii. p. 385. 1829.

up by secondary disease in another person; syphilitic disease of the viscera and nervous centres; syphilitic fungus of the testicle.¹ What wonder, then, if secondary syphilis, polymorphous in its nature, composed as it is of large groups of symptoms, *each of which closely resembles some non-venereal affection*, may have remained long undetected. It must be borne in mind that there are no pathognomonic symptoms to serve as a guide. The history of the case is often the master-key to which we must turn. The surgeon who has a patient under his care from the time a chancre first shows itself to the last evolution of tertiary disease, can easily relegate every symptom to its true cause; the very same surgeon, consulted for a lupoid ulceration, and told by a patient deserving of confidence that he had never had a chancre, might be sorely puzzled to say whether the disease in question was syphilis or not.

M. Ricord, as I understand him, throws out the conjecture, very carefully worded, that possibly syphilis may be the offspring of glanders; a theory which has been repeated by some of his pupils; by one of them as a startling but brilliant idea, though it is just one of those dangerous and seducing errors which, like Darwinism, animal magnetism, etc., are apt to start into life, when a man of great abilities gives up his mind to the illusions of a phantom. Of course, there is no proof, no attempt at it; but, even in pure conjecture, regard to probability is called for. Here there is none. The frightfully fatal nature and rapid course of glanders, the suddenness with which it must have changed from so lethal a condition to one which rarely destroys life, and runs a long, slow career, the utter want of evidence that any similar change has taken place in a disease transmitted from a lower animal to man,—are sufficient to stamp it, in my estimation, as one of the wildest theories ever smuggled into existence by dint of conjectures, and matured into sickly development by the modern progress of “cultivation.” But, were all these reasons against it wanting, dates would alone overthrow it. Glanders, if we are to put faith in M. Beau,² made its first appearance in 1494. Syphilis, precisely as we now recognise it, broke out, at the latest, according to De Vigo, in December of that year; according to the account of that admirable and critical writer, Dr Mason Good, early in the preceding year.

Besides, it is as certain as any fact can be, that the morbus Gallicus was widely spread through Charles's army. After making all due allowance for exaggeration, enough still remains to prove this. Then, supposing the disease to have been glanders in any form, it would have been simply impossible for men to go on fighting and marching when thus affected. They can struggle on for a little while with syphilis, but glanders prostrates them at once. Yet, there is no evidence that military operations were ever interrupted

¹ Recherches sur la Syphilis. Par P. Rollet, 1861, p. 21.

² Robert, op. cit., p. 27.

for a day by any such event. The pages of Daniel, Roscoe, and Prescott contain no allusion to anything of the kind.

As to the importation of syphilis from America, I should not have alluded to it but for two circumstances. It was the invention of a Strasburg physician, of whose narrative Mason Good curtly remarks, that "neither his history nor his arguments are in any degree satisfactory." It was subsequently re-invented by Oviedo, who wrote quite a quarter of a century after Columbus's first return, and who was treated by some of the best historians of his day—Ferdinand Columbus, Herrera, and Las Casas—as a literary Munchausen, the latter declaring that his works "were a wholesale fabrication, as full of lies as of pages," a reputation which has not declined at the present day.¹ And yet, on the authority of a man like this, people were asked to believe that the remnants of the small crews taken out by the great navigator to America—only composed at starting of eighty-two men according to one author, and ninety according to another—could, in the course of a few months, scatter syphilis through large countries like Spain and Italy, even supposing, what is very difficult to suppose, that the ravages of such a malady could escape the notice of so wonderfully observant a man as Columbus.

One of the circumstances, then, to which I have alluded in the foregoing paragraph, is the astonishing fact that the story should ever have been believed at all. The other is, the possibility that, some day or other, we may have the story told again, but this time in another form, and of another country. Dr Thomas Nelson stated before the Committee on Venereal Disease² that, in that immensely ancient country, China, syphilis has existed from time immemorial, and that he found traces of it in Japan. Now, it is true that intercourse with these countries might be described in the fifteenth century as non-existent. Still, China had been visited two hundred years previously, and I believe the communication between it and Japan, though very limited, is of considerable antiquity. Some little intermediate business might have sprung up by the route which the Polos took, and by one of these straitened lines of communication syphilis might have found its way to such a port as Venice; or its home may have been still nearer—in an equally ancient country, like Egypt or India.

Should time ratify the statements made by Dr E. Andrews, we shall have another source of confusion and conjecture. This gentleman, who is Professor of Surgery at Chicago College, says³ that three instances of constitutional syphilis in the lower animals have been reported to him by a veterinary surgeon in that city. The first was that of a bull, who had a chancre on the penis (!), followed

¹ "Among his merits will scarcely be found that of historical accuracy."—History of the Conquest of Peru. By William H. Prescott, vol. ii. p. 44.

² Report, 1868, p. 111.

³ Medical Press and Circular, 10th July 1872.

by secondary disease; the second case was also in a bull, but of a much more doubtful nature; the third was that of a boar, supposed to have a primary sore, followed by eruptions on the skin. He was also informed by the same surgeon, that gonorrhœa is very common in bulls (!). Supposing it to be the case that the bull and boar can acquire syphilis, we have before us the possibility of this disease having been communicated (by contact) from one of the inferior animals. All the old stupid theories about the origin of the disease, the astrological rubbish about its being due to a conjunction of some kind, I pass by without a word. They belong to the days of witchcraft, and there let them lie. But the possibility of one of the two modes of communication just mentioned seems to me worthy of further notice.

But by whatever means it got into Italy and Southern Europe, the fact seems pretty certain that, once there, syphilis, from the year 1494 onwards, spread with great suddenness. I have already noticed the statement by John de Vigo, that, in December of that year, it appeared in nearly all Italy. Twenty-six years later, it had, according to Le Maire, reached over the whole world (!); and even Fracastoro, one of the most learned men of his day, writing in the pontificate of Leo the Tenth, tells that it had then extended over Europe, and part of Asia and Africa.¹ Making due allowance for exaggeration—and we may safely believe there is some here—there is yet enough to show that the disease had made extraordinary progress; a progress which seems impossible of explanation by the ordinary modes of communication, and more akin to the sudden appearance of an epidemic than anything else.

It has been said that the army which Charles the Eighth took with him was the chief or even the sole medium of diffusion; but it is impossible that it could have sown syphilis broadcast in this way. Those who have adopted this theory cannot, I think, have weighed the facts of the case. The number of men did not, at a fair computation, exceed twenty thousand at the time Charles left Ast.² The soldiers supposed to have been the chief instrument in communicating the malady were the Swiss and Germans, and of these only six thousand, principally Swiss, started for Italy.³ I have not been able to make out how the Germans fared, but of the Swiss only twenty-five hundred (including, perhaps, some Germans in the number) left Naples on the homeward march; many, probably, remaining there, as only part of the army was to return, the whole body of troops which then prepared to quit Italy not numbering more than nine thousand fighting men,⁴ who were most seriously thinned down by the battle of the Taro, the painful retreat, and the hardships they endured. Those left behind got on

¹ "Europam, partimque Asiæ, Libyæque."

² Histoire de France. Par le Père Daniel, 1742, tome viii. p. 595.

³ Ibid.

⁴ Prescott's History of the Reign of Ferdinand and Isabella, vol. ii. p. 39.

still worse. Of five thousand men who marched out of Atella, not more than five hundred ever reached their native country. Upwards of four thousand more perished in the island of Procida.¹ We may, therefore, well believe the statement that not more than a fourth of the original army ever returned. The Swiss and Germans were in as bad a plight as any. "They made their way as they could through Italy in the most deplorable state of destitution and suffering."² Yet, although South Germany, if not Switzerland also, was even then thickly peopled, we are told that these broken remnants of a small army conveyed a disease, which must have made them objects of abhorrence to all who saw them, and which would really have invalidated them to the last man before they recrossed the Alps, into "every city, town, camp, village, and cottage."

It has been repeatedly asserted that, when syphilis first appeared, it was, and for several years after continued to be, of a far more formidable nature than it is now, and that it underwent a singular decline, or, as an old author puts it, gradually "abated of its fierceness." I strongly doubt the correctness of both statements. There is nothing in the accounts given by the most reliable authors to show that, taking into consideration the absence of the influence exerted by proper treatment, there was any material difference in the disease as described by De Vigo from what we may see any day in a Lock hospital. I should feel little hesitation in saying that the malignant syphilis spoken of by Mr Walter Coulson,³ and the acute secondary ulceration of which Dr John Morgan speaks,⁴ equal in severity anything related by any trustworthy old author. Besides, I fail to discover in the old writers themselves convincing proof that the disease abated in severity. Fracastoro says it had not improved in his time, and he wrote at a considerably later date; and if we are to put any faith in the monstrous statement of Rosenbaum,⁵ that a Spanish surgeon amputated the penis five thousand times within three months for this disease (!!), the improvement must either have created very little confidence in men's minds, or else have been followed by a bad relapse.

There were, however, certain elements of error present which may easily have contributed to augment the alarm and astonishment excited by the new disease. Among these were phagedæna and sloughing. The former undoubtedly prevailed, and its intractable nature was well calculated to awaken terror both in the patient and surgeon. As to sloughing, we may be pretty sure that wherever large bodies of men get together, and where we find privation, fatigue, and debauchery doing their fell work among them, there

¹ Roscoe's *Leo the Tenth*, 1806, vol. i. p. 361.

² Jovius, *Historia sui Temporis*, quoted by Prescott, *Reign of Ferdinand and Isabella*, vol. ii. p. 61.

³ *A Treatise on Syphilis*, 1869, p. 141.

⁴ *Practical Lessons on the Nature and Treatment of the Contagious Diseases*, 1872, pp. 157, 229.

⁵ Oppenheim's *Zeitschrift*, Bd. xiv. p. 471, quoted in Ricord's *Lehre*, p. 72.

we shall have sloughing. The accounts given of the outbreak of syphilis in Lithuania and East Prussia after the Seven Years' War, that of the same disease in 1806 and 1807, quoted by Simon from Hecker, that by Horn of its ravages in Berlin in 1808, after the campaign of Jena, and numerous reports by army surgeons, completely attest the truth of this statement. With the return to better quarters and food, to more tranquillity of mind and body, comes a cessation of the evil; and possibly this was one cause why the *morbis Gallicus* was supposed to have "abated of its fierceness" after the ill-starred attempt of the French on Naples, albeit the date of the abatement is put much later.

Putting all things together, it is by no means improbable that glanders played its part in the picture so often drawn of syphilis in its earlier days. If we are to credit M. Beau's account, it made its first appearance in the same year (1494) as the siege; and the two affections are so much alike at a certain stage, that they might easily have been confounded then and long after. The first case of glanders I ever saw in the human subject was diagnosed as a "secondary syphilis and delirium tremens;"¹ and M. Robert mentions a case of constitutional syphilis which was mistaken for glanders by several distinguished medical men. At a time, therefore, when the contagious nature of the disease was not known, infection to some extent from such a source was possible enough.

Among the symptoms of early syphilis, we occasionally read of an eruption of hard, painful, burning pustules, as big as acorns, coming out for a long time in succession, bursting with a green discharge, and gradually becoming milder with the lapse of time. This may have been *impetigo rodens* from syphilis, or acute secondary ulceration; but to me it looks as much like the epidemic of boils which followed the cholera of 1849 as anything else. In many cases the boils then wore far more the appearance just described than that of the ordinary *furunculus*, while in others they looked like deep limited sloughs, the surface scarcely raised above that of the skin, and of a pale dirty yellow, fainter than that of wash-leather, the opening being nearly, if not quite, as wide as any part of the sore, and the cuticle, skin, and cellular tissue all apparently melted down into one unhealthy slough. I noted some cases in which I was puzzled at first to make out whether the symptom was due to the epidemic or the syphilis.

We are told that in some cases the hands, feet, etc., dropped off. Admitting the accuracy of this statement, such mutilation could not be the work of syphilis, and was more likely the effect of hospital gangrene or gangrenous erysipelas—a disease which seems to have committed fearful ravages in the good old times, and which,

¹ In the Royal Infirmary of Edinburgh, during the session of 1842-3, when I was assisting Dr Peacock, the Governor of the Infirmary. The true nature of the disease was soon discovered.

according to Sigebert,¹ attacked people in this way. The quaint old writer, speaking of this pestilence in 1089, distinctly says that the victims of this singular malady "either perished miserably, or, deprived of their hands and feet, were reserved for a more miserable life;" while, in an account given nearly seven hundred years later, of apparently the same disease in a modified form, by Dr Wollaston, of Bury,² we are informed that the limbs of several persons attacked by this complaint rotted.

Simon states,³ as if it were a fact established beyond dispute, that in many parts syphilis, the offspring of leprosy (!), has degenerated into a variety of the latter disease. Some of the older writers believed that it could be transformed into tubercular leprosy; others, Paracelsus among the number, thought the morbus Gallicus was a cross between the old venereal disease and leprosy—an opinion not very actively assailed by M. Ricord when alluding to it; and Dr Mason Good describes a form of leprosy which might be mistaken in some of its features for syphilis. As I have not noticed that these opinions have anywhere been critically examined, I propose briefly summing up the evidence for and against the probability of a transmutation which seems to me as impossible as a change of species.

An analysis of the report on leprosy issued by the College of Physicians of London completely disposes of the view that syphilis is ever derived from this disease.⁴ Whatever affinity, whatever similarity there may be between them, there is assuredly no bond of connexion in the way of descent. There is not a jot of evidence to show that leprosy ever degenerates into syphilis or any other disease, or that syphilis degenerates into it. The two diseases may run their course in the same individual: a patient suffering from leprosy may contract either local or constitutional syphilis, and be cured of either or both without the leprosy being affected. Leprosy is as nearly incurable as a disease, curable at all, can well be. No system of treatment can be relied on to influence it in the slightest degree; syphilis can often be cured, and almost always be relieved, by treatment. Leprosy is almost invariably fatal; syphilis rarely so. An infant is scarcely ever born with symptoms of leprosy; syphilis in infants is only too common. The whole list of diseases, therefore, given by Simon⁵ as having undergone this

¹ Elements of Materia Medica. By Jonathan Pereira. Part II., 1840, p. 595.

² Annual Register, 1762, quoted by Pereira, op. cit., loc. cit.

³ Op. cit., p. 3.

⁴ The Pathology and Treatment of Diseases of the Skin. By J. L. Milton, 1872, p. 284. Journal of Cutaneous Medicine, vol. iv. p. 81.

⁵ "Das Pellagra, die Sibbens, die Yaws, die Pians, die canadische Senche, die Krimische Krankheit, das mal rouge de Cayenne, die norwegische Radesyge, die holsteinische Krankheit, die aosturische Rose, u. s. w., mehr oder weniger bössartige Formen der in Aussatz ausgearteten Lustseuche."—Op. cit., p. 3.

suspicious degeneration may be struck out. The phenomena, when analyzed, resolve themselves into leprosy or syphilis, or both : the former, perhaps, dying out ; the latter having possibly undergone much the same change as in sibbens. The history of all such diseases is generally that they have become localized in some out-of-the-way place, among people far removed from their only chance of help, the aid of surgery ; and that these people, judging only from what they could see and feel, have given the disease some local name describing its most prominent symptom. I propose closing this paper with a few brief illustrations of the above statements.

Scherlievo is one of the diseases thus long mistaken. The symptoms are aching pains, hoarseness, difficulty of swallowing, inflammation, and unhealthy ulceration of the velum, fauces, uvula, and tonsils, the ulcers being covered with yellow lardaceous secretion. Having passed through this stage, the disease subsides, and on its reappearance attacks either the osseous structures or the skin. Squamous, moist, and ulcerative eruptions, and *copper-coloured spots about the size of lentils*, accompany those symptoms. The eye, nose, and cheeks are sometimes destroyed by ulceration. "*Growths on the lips*," tubercles, *nodes*, and fungoid ulcers, *ozæna*, contractions of the limbs, and "*fungus of the joints*," are mentioned among the symptoms. The disease is highly infectious, being communicated by contact. What else than misunderstood syphilis this can be, what evidence any symptom gives of pathological change, and what there is which separates it farther from ordinary syphilis than the cases of secondary disease conveyed by the touch, which Dr Colles and others have reported,¹ I profess myself quite unable to discover.

Sibbens was another. This disease, which first appeared in Dumfries about the middle of last century, as described by Freer, Gilchrist, Trotter, and Hill, was marked by an eruption of tubercles like raspberries, covered with a secretion like toasted cheese. In a pamphlet,² now very rare, for a knowledge of which I am indebted to the courtesy of Dr Blacklock of Dumfries, this symptom, from which, I need scarcely say, sibbens takes its name, is described as a fungus much like a rasp or strawberry, elevated one-half above the surface, and, when fully formed, appearing as if set in a socket cut exactly in the flesh to receive it. The disease is said to have been accompanied by loss of the teeth, destruction of the bones of the cheeks, and bright-red painful boils and pustules. Children afflicted with it perished of hunger. The disease was highly infec-

¹ Practical Observations on the Venereal Disease. By Abraham Colles, M.D., 1837, p. 283, etc.

² An Account of a very Infectious Distemper. Dumfries, 1769. There is no name to the pamphlet ; but it was most probably written by Dr Gilchrist, as he published a narrative of the disease with the very same title. If the date of this pamphlet be correct, it quite disposes of the statement by Hill, that the disease was first introduced into Dumfries in 1772 by some infected soldiers, and shows how carefully statements of this kind ought to be received.

tious. Dr Adams, who went to Scotland on purpose to have a look at this mysterious disease, found wasting rather than ulceration of the tonsils, glueing together of these parts by viscid mucus, and pustular eruption, preceded by fever. The disease scarcely, if ever, appeared as a primary affection of the genitals. Its course was much milder than that of genuine syphilis. One young woman, who was watched with great care, recovered in a few days under the use of mercury.

I suppose few will nowadays contest the assertion, that this disease was syphilis. The raspberry-like growths and the fitting of the fungus, as it were, into a socket, have been expressly noticed in other places as symptoms of such forms of infection. Yet Dr Adams concluded that it was a separate malady, and others have shared this opinion.¹ The fact, therefore, that these men should have been mistaken will be ample warrant for assuming that the surgeons of the fifteenth century might easily enough have seen secondary disease in certain stages; they might have seen sibbens without recognising it as the offspring of secondary syphilis, running the course which it usually does when left to itself, but capable, under certain circumstances, of recovering all its ancient malignity.

Some of the symptoms, such as the secretion like toasted cheese, scarcely tally with our ordinary ideas of syphilis. I have only once seen secondary disease assuming a form like that of the growths described above. It was in a young man from Barking, who had, according to his account, which was not very lucid, suffered three months previously from chancre. On the right side of the forehead and scalp were two enormous masses of fungus, covered with a whitish tenacious secretion, somewhat like white kid-leather boiled to a pulp. I tried to get the lad to attend for the purpose of having a drawing taken, but to no purpose, and I saw no more of him. Another dubious symptom is, that the disease never attacked the bones, except by spreading from the soft parts; but I think this is easily explained. Syphilis, thus conveyed, does not often attack the bones, and there may have been an absence of that methodized observation which is now relied on. The third unusual symptom was, that in children the "pustules" chiefly occupied the belly, groins, and sides, so that "all the fatty membrane of the belly below the navel was laid into a large ulcer;" in others the whole scalp was "in a mortified state." In all this, however, there is nothing but what might easily ensue where such a disease was misunderstood and neglected.

Yaws is the last of these affections for which I can find space. It begins, according to Dr Adams, with intermittent fever; after

¹ Mathias, *The Mercurial Disease*, p. xi. Mason Good (op. cit., vol. iii. p. 417 and 419), who thinks sibbens may probably be referred "to such diseases or varieties of diseases as are found to approximate the general character of syphilis, though issuing from a source widely distinct."

the "remission" of this, a universal eruption of pustules takes place, with sore throat and small ulcers on the glans penis. In the case which he describes, fifty-six of these pustules had ulcerated at the end of a month; the sore throat still continued, and the fever was as violent as ever. The scabs which formed on the pustules were horny, and, on removing them, a fungus shot up. The uvula was destroyed. The disease seemed to die out by a process of exhaustion, something like that of smallpox, the whole constitution being affected at one time, and gradually throwing off the incubus. Mercury appeared to exert little control over the affection. Dr Adams confirms the statement of Sleaford, that the "master-yaw," one great pustule, in which the severity of the disease seems to culminate, always leaves a cicatrix.

Unless this disease were syphilis, I am quite at a loss to make out what it was. It may have been a malady which, like leprosy in some parts, has died out. Dr Adams, who was quite in a position to judge, looked upon yaws as a separate affection, as did Mathias; but I incline most strongly to a belief in the identity of the two. Dr Adams contends that the Jews were at one time affected with yaws, and that, about the epoch of the siege of Naples, a remnant of this persecuted race, fleeing from the cruelties of the demons who then held power in Spain, carried the yaws to Rome and Africa. I am not fond of conjecture, but it is difficult to avoid concluding here, that if they took such a disease with them, it was, by the evidence above, syphilis; and this they might easily have done at an earlier date, and in other parts of the world.

Several other diseases of this nature are mentioned by authors—the pians, the mal rouge de Cayenne, the rose of Asturia, etc. The early history of them is obscure, and even the most recent accounts of them are not very clear. Some are leprosy, but, as in the case of the Asturian rose, described by Dr Mason Good, with features so modified by time and distance as to suggest a suspicion that those who first described the malady did so inaccurately. Some are dying out. Dr Blacklock of Dumfries was kind enough to inform me that in a practice there of forty-one years he had never met with a single case answering the description given by Dr Gilchrist; and Dr Borthwick, also of Dumfries, wrote to me that he had never seen the disease in the virulent form described by Gilchrist and others. Dr Daniel, the African traveller, told me that, in a residence of sixteen years on the west coast of Africa, he had not seen more than six or seven cases of yaws, and these he had not examined sufficiently to satisfy himself as to the real nature of the disease; and I have been informed that it is rarely seen in the West Indies, where it was once so common—which, I apprehend, simply means that syphilis is now better understood, and that patients affected with secondary disease are more carefully segregated than formerly.

Dim, however, as is the light so far shed on these strange com-

plaints, it reveals one thing: no transition between syphilis and leprosy, or any other disease—no hybrid affection which could be referred to any possible conjunction of them—has yet been found. As the nebula of one age is resolved by the higher telescopic powers of the next into stars and space, so does the confused picture here, when the beam of critical investigation is turned upon it, resolve itself into the two forms of disease, their natures as unchanged in the long lapse of years as the qualities of matter. When we come face to face with the symptoms, we find that the phantom maladies described by Simon and others have no real existence. They die out, and the twin giants, syphilis and leprosy, reign in their stead.

But faith in these wonderful changes is like divine Ulysses, "not yet dead upon earth." If beaten from the former strongholds, it can take refuge in the shelter which cancer and scrofula offer; and, if properly handled there, with equal success. For there is no proof, pathological or histological, that such a change ever takes place. As cloud lies piled upon cloud, the bearer and its burden equally insecure, so do the theories about these transmutations of syphilis and the facts on which they are built, the fabric and the foundation, prove, when scrutinized, equally intangible and shadowy. I have read in books of the conversion of syphilis, as of other diseases, into cancer, and have heard it spoken of at medical societies; but I believe the evidence amounts to this—that syphilis has preceded cancer in some particular case, and that men believe in the possibility of such a conversion. The belief may be well founded, but I see no evidence that it is so. Similarly, I have heard and read that syphilis, after it has percolated through the tertiary stage, may reappear in the children as scrofula. That it may be an occasional factor in the genesis of this disease, I do not deny, for I have no facts to guide me on the affirmative side of the question; but I know that I have carefully inquired into the history of scores of cases of scrofula, without once tracing it to such a source. One author has asserted that struma is always the offspring of syphilis. This means, in plain English, to brand thousands of fathers, men of the strictest morals, with an ineffaceable stigma; and, to speak plain English about it, I should take the liberty of describing it as a monstrous and unfounded slander.

ARTICLE II.—*Cerebral Hæmorrhage mistaken for Hysteria.* By JAMES CHRISTISON, M.D., Preston.

SOME few weeks ago, I saw a note in the *British Medical Journal*, of a case read before the Obstretrical Society of Edinburgh, in which cerebral hæmorrhage had been mistaken for hysteria. I have not been able to find any detailed account of the case

published, and am therefore unacquainted with its history and course, what the prominent symptoms were, and how the mistake in diagnosis came to be made.

Having, however, had a case under my own care lately, in which the same error was made, I have thought that it would not be altogether uninteresting to your readers, if I laid the main facts of it before them. They are, I am sorry to say, only of a meagre character, the patient being one of those erratic customers whom it is impossible to watch for any length of time, and who only come under the observation of their medical attendant when urgent symptoms absolutely compel them to do so.

The patient was a woman, thirty-two years of age, unmarried, but the mother of three children, the last of whom was born some four years ago. She was a thin, pale, anæmic-looking subject, of a highly nervous temperament, and having a worn and old looking face.

Since her last confinement, she had never enjoyed anything like good health, and had suffered from complete amenorrhœa, never having had the slightest show of menstruation since then, but, at intervals at what she thought might be about her menstrual periods, she had copious bleeding from the mouth and nose.

Something like two years ago,—I am not certain as to the exact time, as I quote from memory,—I was called to her, and was told that while at work in a cotton-mill, she had been taken suddenly ill—had what her fellow-workers thought was a fit. I found her in bed; she had been able to walk home with some assistance; partially insensible, answering questions only when loudly spoken to, and then in a rambling, stupid manner, muttering and talking to herself almost continually when not sharply addressed at first, and then after a time lapsing into a state of semi-unconsciousness, from which she could with difficulty be roused. The temperature and pulse showed no appreciable alteration from the normal standard; and, in fact, she presented more the appearances of a person in a condition of partial hysterical catalepsy than anything else. She lay as if in a quiet, composed, easy sleep; the breathing was perfectly natural and quiet, and there were no appearances whatever to indicate any serious disturbance of any important organ.

Next morning, I found her perfectly sensible, complaining of a little headache and feeling of languor, but otherwise apparently not far removed from her usual state of health; and, in fact, so soon did she get well, that she was out on the third day, and back to her work again by the week's end.

Twice since that time, I have seen her under precisely similar circumstances, the attacks having come on in the same way, been of the same character, and the recovery on each occasion as rapid as it was on the first. At neither time was I able to find any explanation of the attacks; there never was any trace of albumen in the urine, and it was generally rather more copious than usual

for two or three days after each attack, and the heart sounds were always perfectly normal.

The amenorrhœa had continued during the whole of the time, and she would never submit to any treatment directed toward re-establishing the menstrual flow.

On 5th February, at 12 P.M., I was asked to visit her, the messenger informing me that she had died suddenly in bed. As my presence could under the circumstances be of no possible use, I declined to go then, but called next morning, and was informed that she had gone to bed about 10 P.M., apparently in her usual health, and about an hour and a half after, her sister, on going up stairs, had found her insensible, and she had died almost directly.

I made a post-mortem in the afternoon.

On removing the skull-cap, it was found to be adherent at two or three places to the dura mater; and, at two points close to the longitudinal sinus, on the left side, there was a hard gritty deposit in that membrane.

There was slight venous turgidity on the surface of the cerebral hemisphere, and on the edge of the third left frontal convolution a small clot of blood showed itself. Between the clot and the longitudinal fissure the brain substance felt exceedingly soft; and on gently attempting to separate the convolutions to trace the clot inwards, it gave way, and exposed a large cavity filled with coagula, a considerable portion of which was recent; and in the centre lay a hard clot the size of a small walnut, surrounded by a grumous-looking fluid. This clot was evidently of some considerable age, as it was very hard, and required some pressure to cut it with a knife.

The rest of the brain was healthy, with the exception of a small recent clot, about the size of a bean, in the posterior part of the right hemisphere.

The cerebral arteries were the subject of degeneration.

I was not permitted to examine the rest of the body.

The principal point of interest in the case appears to me to be, the difficulty of arriving at a correct diagnosis of the cause of the attacks.

The age of my patient, her general condition, history, highly nervous temperament, the existence of amenorrhœa, and the character of the attacks,—all led me to the conclusion that they were hysterical.

That there must have been extensive effusions previous to the fatal rupture, is evident from the size of the old clot, and it is really a subject of wonder to me, how the woman could have shown such slight symptoms, and recovered so rapidly as she did on each occasion, considering the amount of injury her brain must have sustained.

It is of course a well-established fact, that effusion into the upper portions of the cerebral hemispheres is attended by symptoms of a

less decided character, and, *cæteris paribus*, less fatal, than when it occurs in any portion of the base or near the medulla; but I do not remember ever having seen or read of a case where the hæmorrhage was so extensive, and the consequent brain lesion so great, and the symptoms so comparatively slight, as they were in this case.

It is also worthy of remark, that although the third left frontal convolution was involved in the injury, there were never at any time symptoms of what is called aphasia; the power of expressing herself clearly and distinctly was, on recovery of consciousness, always perfect; neither was there the least loss of memory noticed. Microscopical examination of the clot did not show any blood-crystals, but probably that was owing not to their absence, but to the inability of the observer to detect them.

ARTICLE III.—*Remarks on Insanity.* Part III. By STRETHILL H. WRIGHT, M.D., Medical Superintendent, Barnhill Hospital and Lunatic Asylum, Glasgow.

IN the numbers of this Journal for March and May 1873, I ventured to publish "Some Remarks on Insanity." I would now make a few concluding observations. I desire in the following to direct attention alone to individualities of an "insane" peculiarity; and to propose certain groupings of such individualities which seem to be of use as regards the treatment, or rather disposal, which such individualities demand.

Before entering upon these concluding remarks, I crave indulgence while I cast a retrospective glance on what has been already stated as regards the matter.

I beg that it may be borne in mind that these "Remarks on Insanity" only refer to a certain class of the "insane," though doubtless that class is a very large one. Only those "insane" come under the scope of these notices whose mental peculiarity is a true evolution of that individual peculiarity which is congenitally, or has become, intrinsic to their organism. In a paper published in this Journal for Sept. 1872, I drew attention to a form of "*asthenic*" insanity, where a constitutional vigour below that of the average of mankind, and a tendency to abnormal mental action, were congenital. Asthenic action was the natural outcome of an *inherited* want of capability for more robust manifestation, and by no means could be truly looked upon as arising from the action of extrinsic influences. Such influences might render the imperfection of the organism more obvious, but the incapacity for action of a normal sanity was inherent in the constitution of the individual.

I think, in the majority of such cases of congenitally insane individuality, *there is no want of vigour* in the manifestations of the individuality. The individuality frequently manifests itself with

intensity. The functions of animal life are usually vigorously performed. But the individual peculiarity which is more or less noticeable in the doing and present in the consciousness of every man, and which stamps each man as a special creation, in certain men is *such* that their mode of action can by no means be made to fit in with the doings of their fellows. They are, therefore, held by their fellows as necessarily to be debarred from participation in the business of the commonwealth. The sense of the pitch of peculiarity at which the individuality of such insane organisms displays itself leads to the application to them of various epithets indicative of the general sense of the peculiarities of their individuality.

In the first of these papers, "Remarks on Insanity," I submitted some considerations on "the insane individual in relation to the individualities around him." I strongly urged that "there is no sharp boundary line between 'sane' and 'insane.' The measure of insanity is a question of individual evolution. Although it is easy to recognise the wide gulf between the abnormality of a markedly '*insane*' individuality and the normality of an ordinary '*sane*' individuality, still there are numerous individuals of a peculiarity gradually increasing, till one is forced to allow that this individuality is of insane peculiarity."

In a second paper, entitled "Remarks on Insanity," I went on to consider—(1.) The growth of the insanely peculiar individuality, —while considering this view of the matter, I presented three modes in which the insanely peculiar individuality might be developed: (a.) by *gradual* evolution among the progenitors, through generations; (b.) by the influence of a specific period of ill-health affecting the parent while concerned in the procreation of the offspring, a condition of ill-health which might be entirely abnormal to the original constitution of the parent, transitory, and non-recurrent; (c.) by the influence of injury or constitutional disorder occurring in the life of the patient himself—by the influence of such injury the constitution of the person is warped from the tendency of its original development, and thenceforward develops itself in a manner abnormal to its original bent. (2.) I then made some remarks as to the mode in which the possession of such an insanely peculiar individuality affects a man's course through life.

I would now in the following speak only of those whose individuality is of such abnormality as to render its possessor the subject of special observation, and render him obnoxious to the epithet "*insane*," applied to him by his fellows.

I shall present certain great divisions under which it seems insane individualities may be advantageously and usefully considered.

It must be always carefully borne in mind that such groupings—that, in truth, all classification of natural phenomena—are but watch-towers to aid the intelligence in gaining a somewhat clearer view of the *continuity* of the matter: in point of fact, it will be

found that there are no absolute natural demarcations of mental phenomena into classes; but certainly there are cases of insane individuality of so strongly marked features that they may be advantageously held as types of a family,—certain other cases will be found from point to point to bear a strong family likeness to these typical cases—a likeness which, in other instances, becomes less marked, gradually fades, and is replaced by other features, which in turn, by their gradual pronunciation, approximate to the typical cases of another group. Truly insane actions are alone recognisable as “insane” from a review of the circumstances of their production. Any one action of an insane person might, under certain circumstances, be the action of a sane man. The verdict of sane or insane in relation to any course of action is decided upon from a consideration of the adequacy of the exciting causes which produced such action. If the exciting cause were such as to justify the course of action taken upon it, then were such action the conduct of a sane man; but were the course of action notoriously unproportionate to the influences which produced it, it could not but be considered as the action of a man whose organism did not comply to the ordinary standard of what is considered “sane.”

I shall now present certain great divisions under which it seems insane individualities may be advantageously and usefully considered. Where, then, “insanity is the result of the natural evolution of the individual organism,” a somewhat clearer view of the matter seems to be afforded by grouping such individualities as follows:—

To Group I.—belong those individualities whose manifestations are, with slight fluctuations, of uniformly insane peculiarity. To Group II.—belong those individualities which do not manifest uniformly insane peculiarity, but in whom insane peculiarity is manifested at more or less regularly recurring periods. To Group III.—belong those whose individuality manifests itself as of insane peculiarity at certain so-called critical periods of life. To Group IV.—belong those individualities which are probably of more or less constantly insane peculiarity, but whose peculiarity only reaches an obviously insane pitch under special pressure.

I shall now proceed to quote cases typical of the groups to which they belong. The letters used to designate the following cases are not the true initial letters of the individual's name, but are merely used to distinguish the one case from the other.

No. 1.—Case of N. R.—I quote extracts from this case, partly as it is reported in the case-book, but also partly condensed as seemed best to present its characteristic features.

“N. R., female. Well educated in ordinary branches. Natural disposition cheerful. No industry, and most unsettled. Constantly ailing, but no particular disease. Insanity generally in the family. No bodily disorder preceded the attack.”

"The ideas are most abundant, wandering, erroneous, and absurd, and not much under control; and in conversation her manners are extremely mischievous, violent, and excited. She had at times various delusions. On her first admission to the Asylum, some time previously to the above, it is said of her, 'Her husband states that she has been a burden and vexation to him for years, being intemperate, untruthful, suspicious, jealous, and menacing; he thinks his life in danger from her, and has tried in vain every other method of treatment.'" This woman is a very excellent example of the insanely peculiar individual whose "insanity" is constant. She is of an insane family; she has herself been all her life insane. The insane peculiarity of her nature is betrayed in every action she performs, and in every sentence she speaks.

Take another note regarding her:—"N. R. is a great nuisance in the gallery. She obtrudes herself on every one's society, and cannot be made to understand that her society is not desired." When N. R. suffered from any little bodily disorder; when she underwent any little annoyance; when she joined in social gatherings, and strove to recommend herself to the notice of others, she manifested herself with an appropriately insane and most disagreeable peculiarity. In her illnesses, mostly feigned, she was insanely apprehensive; exigent of attention, she was always haunted with a morbid desire to attract notice; utterly selfish, she was at once huffed if she thought any one preferred before her; an elderly woman, she caricatured the archness and coquetries of youth; when removed from asylum restraint, she constantly transgressed the most obvious obligations of social life.

This person's case, then, affords an example of an insane individuality, produced probably by gradual evolution among the progenitors. "Insanity generally in family." She was herself more or less constantly insane. She displayed no delusions of a very extravagant kind, for she was possessed of but a limited mental capacity; but as far as her mental manifestations went, they were constantly of insane peculiarity. On reviewing the case, I think the reader will be struck how this woman's insane peculiarity presents in an exaggerated form the disagreeablenesses of many persons met with in ordinary life. It sometimes happens that men marry wives who are a burden and vexation to them; who are intemperate, untruthful, suspicious, jealous, and even menacing—and can do with them. But this woman's unfortunate individuality presented all these traits in a form so exaggerated that she was pronounced insanely peculiar and unfit to play her part in the commonwealth, and was secluded from it. It is quite possible to conceive, or rather select, individuals whose lives are a torment to all about them, but who are not secluded; persons who clog the wheels of social life, or obstruct the business in which they are engaged, by their untruthfulness, unreliability, etc.

The above case, then, is taken as a sample to aid somewhat in the

illumination of the matter in hand—i.e., Group I., to which belong those individualities whose manifestations are, with slight fluctuations, of uniformly insane peculiarity. Many persons, the nature of whose insane individual peculiarity would draw them within the range of this group, possess intellectual powers of a very high order, not the highest. At present we speak principally of those whose individual peculiarity is such as to render their seclusion from the commonwealth necessary.

It is also a matter for serious, and it ought to be a subject of anxious, consideration—Can these persons be got to exercise what faculty they possess? Because their individuality is of insane peculiarity, does it follow of necessity that they are unable to do any work in their generation? It is true, many are unable to discharge duties as responsible members of a community, because they are unable to regulate their own mental operations in the discharge of the business of life. But many insanely peculiar individuals are possessed of *special* powers of a high order; also, though possessed of an insane peculiarity, they are not *therefore* deprived of *all* power of doing sane work. The insane peculiarity may be of all shades. Many such “insane” lead lives—conduct themselves in a way which commands the respect and esteem of those in charge of them—some by the patience with which they bear a lot without hope, at least in this world; some by the righteous way in which they struggle against the infirmities of their nature.

The case of J. W. may be taken as a fair sample of Group II. “To Group II. belong those individualities which do not manifest uniformly insane peculiarity, but in whom insane peculiarity is manifested at more or less regularly recurring periods.”

In the following, I have endeavoured to present extracts from the account of this case, which truly, yet still as shortly as possible, embody the most salient and characteristic features of it. The case is very fully reported in the case-books. The dates of successive attacks of mental disorder, “insanity,” are noted with so considerable an accuracy that it is thought that the conclusion drawn from their consideration is reliable. The characteristic facts of this case, then, present themselves as follows:—

It appears that J. W. first manifested an insane peculiarity at about the age of twenty-one. It is stated that there are no insane relatives; that the cause of insanity is excessive mental and bodily exertion—want of sleep. When admitted to the Royal Edinburgh Asylum, he had experienced attacks of mental disorder intermittently for the space of eight years. During these eight years twelve attacks had occurred. On admission, it is further reported of him as follows:—On an occasion previous to his admission to the Royal Edinburgh Asylum, it is noted—a note of great importance—“he admits a state of excitement, during and owing to which he can get no sleep, and experiences an uncontrollable disposition to shouting, singing, and sudden violent movements.”

On reviewing the dates of his successive attacks, it would appear that they bear a certain relation to periods of a month. Thus, between the dates Dec. 12, 1867, and Jan. 22, 1869, it is noted as follows:—Dec. 1, Becoming ill again; Dec. 24, recovered; Jan. 24, 1868, another attack; Feb. 11, better; March 14, another attack; May 1, recovering. An interval of two months. Aug. 13, another attack; Aug. 18, recovered; Sept. 24, another attack; Oct. 11, recovered. Apparently remained well till January 22, 1869, when he underwent another attack.

Another extract:—"His insanity appears to be closely connected with the sexual organism; for when the seminal fluid comes away by nocturnal emissions he keeps comparatively well; if it be retained for more than a fortnight, symptoms of excitement are almost sure to come on. The maniacal state has never lasted for more than three weeks, nor for less than eight days, and during it he whistles, sings, and presents the ordinary symptoms of mania."

Again:—"During its course (the maniacal excitement) his natural manner and disposition were completely changed. From being quiet, gentlemanly, and almost reserved in his manners, and scrupulously correct in his conversation and conduct, he became excessively talkative and obtrusive; his voice loud, his manner restless and excited; his mind, too, was obviously occupied by thoughts and ideas from which he would have shrunk when well."

After one of his attacks, if a severe one, his bodily condition is much reduced; his appetite is great, and he eats largely till he recovers his ordinary condition.

The facts of the above case may be summarized as follows:—The case of J. W. is one where excitement of the mental operations occurs at more or less regularly recurring periodic intervals. Where this mental excitement seems to be closely connected with the generative function, the first manifestations of the coming excitement are a feeling of restlessness and inability to sleep: gradually the mental excitement increases, and involves all the mental operations. Self-control is entirely in abeyance, and the patient manifests himself as quite a changed man—from being a quiet, unobtrusive, well-conducted gentleman, he becomes a loud-spoken, obtrusive, evil-speaking person, while in this state of excitement.¹

It would seem then that, for the last twelve years at least, this individual has at times manifested himself with an insane peculiarity; this insane peculiarity would appear to be closely connected

¹ In regard to Group II., I would merely further remark that the passing of the organism into its insane phase is usually gradual: it is ushered in by a general restlessness, a failure of sleep, and an inadvertence to the discharge of special bodily functions. The individual frequently experiences a sense of increased mental power; and it is indeed matter of fact that, for a time at least, there is an exaltation of the mental faculties, which shortly gives place to that exhaustion which follows of necessity an overtasking of the brain, or, indeed, of any organ.

with the generative function. It was remarked, while speaking of the mode in which the possession of such an insane peculiarity affects a man's conduct through life, in a former notice, that man's body, for the purposes of the present, might be described as consisting of three cardinal parts—"one part the generative apparatus for the continuation of the species." Now, on reviewing the facts of J. W.'s case, it would appear that here is an individual possessed of an organism more or less sensitive to impressions—the generative function tends to manifest itself in an emphatic manner at certain intervals of time; if, probably from recondite causes, it is unable to discharge its function, it produces increasing perturbation of the general organism; nervous irritability ensues; sleep fails; the general mental susceptibility to impressions is increased, becomes exaggerated, shows results in such an uncontrolled expression of the ideas produced by these impressions, that the patient himself is conscious of the want of control over his mental operations; and those about him consider him, while in this state, as quite a changed man. After the period of excitement is passed, it is succeeded by great languor and prostration of all the functions, mental and bodily.

The chief points of interest in this case are—1st, That the patient himself "admits a state of excitement during, and owing to which he can get no sleep; and experiences an uncontrollable disposition to shouting, singing, and sudden violent movements; and, 2d, That these attacks of excitement occur at more or less regularly recurring periods, and are connected with the generative function. In regard to the first point, it would seem that this individual was gifted with a certain organism, and with a certain power of controlling it. Under the ordinary circumstances of his life his self-control is perfect; but under the extraordinary circumstance of interference with the due operation of the generative function, the action of the whole organism is by degrees so modified, that his self-control is not sufficient to retain its workings within the bounds of sanity. When the vehement action of the brain and nervous system is reduced by absolute exhaustion, his powers of self-control again re-assert themselves, and duly regulate his mental manifestations till the disturbing cause again comes into operation. In reference to the second point—the regularity with which the sexual excitement occurs in this case, is, I think, noteworthy. It is, I believe, by no means unfrequent to find a well-marked periodicity in the sexual manifestations of the male as well as the female, though I am not at present prepared to bring evidence to bear on this point.

J. W. then manifested himself, at short intervals of more or less regular recurrence, as of insane peculiarity; his insanity was traceable to the disordered operation of a specific part of the organism. In many cases of periodic insanity the periods are much longer during which the individual manifests both a sane and an

insane individual peculiarity; in many it is impossible to date the insane peculiarity from disorder of any special part of the organism. The claims which such cases as these last have to be included in Group II. are, that whatever the cause of *insane* manifestations may be, it is intrinsic to the organism; it operates upon the general working of that organism at more or less regular periods, so that the insane phase of the individual's life must be considered as the natural outcome of the influence which the various parts of his organism exert on each other in the discharge of their relative functions.

I would now pass to the consideration of those cases which appear to range themselves under Group III.; in which group are included "those whose individuality manifests itself as of insane peculiarity at certain so-called critical periods of life."

Now, at certain ages, certain physiological changes take place in the human body: the constitution passes through certain crises, so to speak. These symptoms of general constitutional modification are, in many instances, traceable to changes affecting the generative function; but it does not seem at all established that changes in this function are fundamentally concerned in all cases. It would rather appear as if the modification of the generative function only formed a part of general constitutional change; although, no doubt, symptoms arising from modification of the generative function are frequently strongly marked, and exercise an important influence on the mental operations of the individual temporarily or permanently more or less. The times at which these physiological changes most markedly occur are three in number. The exact date of their occurrence varies as to a year or two, but may be stated with sufficient exactness as follows:—The age of puberty occurs from the 14th to the 17th or 18th year—the generative function of the individual reaches its potential development. The age of completed organic development occurs between the ages of 25 and 30—the organism at this period attains the final development of its animal vigour. The period of the grand climacteric is of somewhat more uncertain date, but it may be held that between the ages of 38 and 56 certain constitutional changes occur, and are more or less well marked in different individuals; these show evidence of their operation in the cessation of the menstrual discharge in the female, and less palpable but still sufficiently evident symptoms of organic change in the male. Now, in certain persons, disorder more or less well marked of the nervous system and mental operations occurs as symptomatic of the physiological changes in operation at these periods. Want of space forbids the adducement of examples of mental disorder occurring at each of these periods; but I present a short account of well-marked mental disorder displayed by an individual at the period of the grand climacteric. This person seems to have come of an insane stock, as she has a half-sister notably insane, whose insanity came on at the same period of life. I quote from the records of the case:—

"Mrs T. was first attacked at St Andrews, and went to Edinburgh for two months; was then about 43 or 44 years of age; has been insane 15 months in all. Cause, sudden death of husband, three years ago. Not epileptic, suicidal, nor dangerous. Symptoms of insanity, refuses to answer any questions, or to take a sufficient quantity of food for her bodily support. At the commencement of her illness, was very suspicious of her food being poisoned." Again:—"Is sullen, and cannot be got to converse, or even speak a single word. Is rather sleepless. Mrs T. is rather an interesting case; her brother was insane. There are no symptoms of the suspicion mentioned in her warrant; she is very variable. Her prevailing cast of mind is one of melancholy, and she has a great aversion to allow the furniture of her rooms to remain two days alike." About a month afterwards, "Mrs T. still persists in staying in bed. It is only about one day in four she can be persuaded to get up." Again, six months afterwards:—"No change, except that Mrs T. has of late taken a fancy to swear, which she does constantly, using very low expressions."

Mrs T., as ascertained by private information, during the part of her life previous to this manifestation of insane peculiarity, conducted herself with remarkable self-control and wisdom in the relations of her life. She appears to have come of an insane stock—a half-sister and brother were insane. At the period of her grand climacteric she experienced a severe mental shock in the sudden death of her husband. This occurred three years before manifestations of insanity made their appearance. From the facts of the case, it would appear that Mrs T. inherited an individuality which manifested itself as of insane peculiarity at the period of her grand climacteric. It is a question to what extent grief, in consequence of the sudden death of her husband, was instrumental in producing insanity in this case; or whether the manifestation of insane peculiarity was not chiefly due to the operation of physiological causes. A half-sister became insane at the same period, without any obvious reason for insanity in her case. Sometimes, in these cases, where the "individuality manifests itself as of insane peculiarity at certain so-called critical periods of life," the mode in which the insane peculiarity manifests itself is of special significance—it may be distinctly traceable to changes affecting the generative function. Such significant changes are more frequently found in insane peculiarity occurring at the period of puberty; but the distinctive features of any one person's insanity are, in the main, those of his sane individuality manifesting itself with exaggerated force and without control. Excitement or depression, I think, are never found as the sole evidence of insanity; cerebral excitement or depression may be the prevailing cast of insane peculiarity, as a tendency to excitement or depression may be the prevailing cast of the individual peculiarly manifesting itself within the bounds of sanity. It is true, no doubt, that the person of melancholic dispo-

sition, if he become insane, will more frequently exhibit symptoms of exaggerated melancholia, as the person of an excitable temperament will manifest symptoms of mental excitement under the same circumstances; but it may, I think, be held with truth that where insanity is the natural evolution of the individual organism, neither insane melancholy nor excitement can be held as characterizing insane mental peculiarity as manifested at any one period of life, or as distinctive of any of the groups of which we at present speak. Nay, more, insane excitement or depression are often manifested by the same individual; they are found to alternate with each other, and by no means unfrequently a state of mind which seems well to be characterized by the phrase "Acute Melancholia," is associated with acts of great violence, with an inveterate suicidal tendency, and with great perturbation of the whole nervous system. Special mental manifestations may be traceable to the special circumstances of the case; but it must be always remembered that the general cast of insane mental peculiarity is that of the original disposition of the individual while manifesting himself in a manner recognisable as sane.

Lastly, to Group IV. belong those "individualities which are probably of more or less constantly insane peculiarity, but whose peculiarity only reaches an obviously insane pitch under special pressure." The pressure, the circumstances which urge the susceptible temperament into the manifestation of insane peculiarity, may differ in kind, may vary in gravity of effect, according to the tendency to insane action of the temperament on which they operate. The extent to which any such individuality may, under such pressure of circumstances, manifest itself as of insane peculiarity also varies greatly. One person will merely exhibit general mental excitement—inability to pursue a train of thought—an unwonted garrulousness and incoherence in his conversation; another will exhibit symptoms of the most violent mental excitement, utter incoherence of ideas, an excessive demonstrativeness in speech and action. According to custom, I present a case which may serve as a specimen of the group to which it belongs.

The Case of D. B.—This individual has been more than once insane. On every occasion his disorder seems to have been produced by business anxiety in great measure. I believe his domestic life was not a happy one. I quote extracts from his case on his first admission to Morningside:—

"Possesses a considerable amount of house property, the business of which he has hitherto transacted with great acuteness."
"The present attack has existed for three months. Neither suicidal, epileptic, nor dangerous. No H. P."

"On admission he was greatly excited, shouting and swearing. Said he could bring down the heavens and destroy every one." He was discharged recovered at this time, after a residence of rather more than two months, in the course of which he had been

liberated on pass, but was brought back, having exhibited symptoms of great mental excitement during his absence.

Again, on his second admission, three years afterwards :—“D. B. has been a patient here before, at which time his attack lasted only a short time; and he was discharged on pass, but was brought back before the time of his pass expired in a state of acute mania. He is now convalescent from the attack for which he was admitted, and was to-day discharged on pass with an attendant.”

A fortnight after :—“D. B. to-day returned. He has remained perfectly well since he left, and has transacted some important business. No symptoms of insanity could be discovered, and he was discharged recovered.”

The above, then, is the case of a man who achieved for himself a very considerable position by the exercise of the eminent business faculties of which he was possessed. He under ordinary circumstances conducted the business of life in a manner recognised as sane by those about him; but, under the pressure of circumstances, mental irritability was produced, gradually increased, till finally he displayed himself as of very insane peculiarity. He was violent, used foul language, and became oblivious of the ordinary requirements of decency. A very short seclusion from the business annoyances which produced his mental disorder sufficed to re-establish his equanimity; indeed, during the greater part of his stay in the asylum he conducted the arrangements of a somewhat extensive business without any assistance from others. Except on the few occasions on which he underwent circumstances of special annoyance, he exhibited himself as of “sane” individual peculiarity; but when exposed to exceptional embarrassments, his not perfectly sound temperament gave way, and he exhibited a flaw in his organism by the manifestation of “insane” peculiarity.

Every man who has led a life of any mental activity, or has been concerned in the conduct of responsible business affairs, must have experienced occasions when his overtasked brain ceased to reply with its wonted steadiness and precision to the calls made upon it. Instead of the firm grasp which he ordinarily gained of a matter, his apprehension, by over-exertion, was more or less impaired, and he found himself tending to the forming of inconsiderate judgments, hasty, ill-advised conclusions, with their result of unsuitable, unjust action. He tended to become irritable, captious; and felt himself unreliable. After a short *rest*, his mental balance recovered, and he found that he was able to conduct himself truly as before. The same was the case, in more marked degree, in regard to the above insanely peculiar individual. When too great a stress fell upon his organism, its action became so disordered in every part of it as not only to render him incapable of discharging his business, but to necessitate for a time his seclusion from the commonwealth.

In these three notices—“Remarks on Insanity”—I would, in conclusion, remark that the insane man has been considered—1st,

as regards the relation which he bears, *quoad* his insane individuality, to his fellow-men around him; 2d, as regards the *evolution* of that insane individuality which renders him a man conspicuous, endowed with which he is a specially-weighted runner in the life-race; and, 3d, in this last notice, as regards the manner in which and the extent to which his infirmity disables him from acting as an ordinary member of the commonwealth.

Some persons are constantly "insane." They *never* conform to the standard of what can be called "sane."

Some, for more or less prolonged periods, are fitted to mix with their fellows in the social concerns of life; but at certain times their organisms pass through an "insane" phase, and their individuality is then manifested as of such insane abnormal peculiarity as to necessitate their seclusion till their organism has passed through its "insane" phase. The most noteworthy fact in relation to such insane persons is, I think, that the "insane" action of the organism is frequently traceable to the extending influence of the exaggerated action of some one portion of that organism. Periodicity is a well-marked feature in the eccentricities of such organisms—a periodicity which seems to arise in very many cases from recurrent physiological modifications of a recondite nature—modifications which, though in many cases they are extremely well marked as to their occurrence, still cannot be traced to changes in the operations of any distinct part of the organism. The case of such persons is one of great interest, as during a part of their lives they entirely conform to the ordinary standard of sanity, are able to engage in intellectual and other pursuits; nay, more, if engaged in any pursuit suited to their capacity and agreeable to their tastes, their mental capacity may become so engrossed that their insane phase may be warded off, and the physiological changes which would lead to it may transact themselves without interfering with the balance of other parts of the organism.

When insane peculiarity manifests itself at climacteric periods, the frequency of a return to the normal standard of mental stability varies according to the age of the individual. When insanity occurs at the periods of puberty and the completion of animal vigour, the chances of improvement are greater than when insanity makes its appearance at the period of the grand climacteric, when there is a want of power in the organism to correct erroneous action in any part of it; yet in many cases, where irritability is the most marked feature of the "insane" manifestation of an individual at this time, it not unfrequently happens that the mind recovers its equilibrium as age advances. "Insanity," both as regards this, and also the final group of those defective organisms which display themselves as of insane peculiarity under pressure, is merely a question of degree. A change of which the individual is himself conscious, which may indeed be noticed by his friends, comes over many a man at the period of his grand climacteric. His sun may

pass through a lowering and stormy sky as it commences its declension, but, nevertheless, as it advances to the setting, may cast back a flood of illumination on the life-landscape which lies behind, and advance to its departure through a sky calm and free from clouds, or mellowed with the golden memories of storm and struggle past.

Finally, after a review of such "insane individuals," the great question urges the reviewer—What is to be done with such "insane" persons? Some, though constantly "insane," are still able to do some work in their generation. Some are possessed of little more than an ability to obey the simple behests of sane minds. Some there are whose "insanity" consists in a deficiency in that force of character which enables a man to fight his way in the general *mêlée* of life; but when taken care of, and afforded opportunity to unfold themselves, they display the most unexpected capacities and endowments. Some there are, again, who require gentle restraint and protection from the infirmities of their own organism. Such varieties are endless: the varieties of individualities. One advantage seems to be gained by the distinction of insane organisms as divided into—those whose insanity is the result of special diseased conditions of the organism, and those whose insanity is the outcome of the natural evolution of their organisms. It facilitates the clearly recognising when "insanity" can be cured, and when it is only to be made the best of. If the "insanity" depend upon temporary diseased conditions, then it may be hoped that, if the organism can be induced to rally from its sick state, the mind also may again manifest itself after its normal sane fashion. If the "insanity" be a natural manifestation of the organism in certain phases of its existence, a cure of such falls under the same category as the attempt to compel the Ethiopian to change his skin or the leopard his spots. The question with such is,—Can any useful work be got out of such persons?—how to modify their surroundings, as at once to satisfy the two great requirements—non-interference with the weal of the sane community—the greatest amount of happiness for the insane individuals.

ARTICLE IV.—*Note of a Case of Secondary Syphilis communicated by an Infant to its Nurse.* By ANGUS MACDONALD, M.D., F.R.S.E., Lecturer on Midwifery and Diseases of Women and Children.

(Read before the Obstetrical Society of Edinburgh, 8th January.)

THE present communication makes no pretence to being a paper in the ordinary sense of the term, but is simply a note upon a case of considerable merit, as bearing upon a class of diseases which are interesting alike to the physician and the obstetrician. It is shortly as follows:—

Mr D., residing in Edinburgh, and aged 26, married, in July 1870, a woman about his own age, and who was up to this period perfectly healthy. Mr D. had about eighteen months previously to his marriage contracted a venereal affection which he believed to be a gonorrhoea, which was followed by an eruption like measles, and by falling out of his hair. But he was unaware, or at least states that he was unaware, of any primary sore. At the time of his marriage, and ever since, he was and has been perfectly well, and has never exposed himself to the possibility of contagion. About three months after marriage, his wife began to suffer from sore throat, and at the same time her skin became covered with an eruption. She likewise caught a severe attack of bronchitis in December of 1870, for which I was called in to attend her. This illness proved of an exceedingly severe character, and continued, though with abated intensity, till after she gave birth to a putrid child on 5th March 1871. So far as appearances and circumstances warranted an opinion, this event happened in the seventh month of the period of utero-gestation. From this time onwards she continued to improve gradually and steadily. Her sore throat and her bronchitis disappeared, and she began to gain flesh, until, after a residence of some duration in the country during the autumn of 1871, she returned to town in perfect health. I may state that when summoned first to attend her, I found her suffering from sore throat, presenting all the appearances usually ascribed to mild syphilitic sore throat, and, besides, from a corresponding skin eruption. There were also a number of rupia-looking ulcerations throughout her body, and markedly so on the external aspect of the labia pudenda. The strictest inquiry resulted in showing that there never had been anything of the nature of a primary sore on the genitals.

With her improvement in general health, these skin affections and the sore throat also disappeared. She meanwhile had become again pregnant, and on the 11th of February 1872, she was delivered of a male child at full term, who seemed at birth quite healthy. Exactly a month after her confinement (which, by the way, passed off quite regularly, and from which she made a good recovery), I was asked to see the child, which had been suddenly attacked with a skin eruption. This I found manifestly of a syphilitic character. It soon became general over the whole body. The mucous membranes of mouth, eyes, nostrils, and ears, became rapidly involved, and poured forth their secretions. The snuffles were specially intense and distressing. The infant for a time could rest neither night nor day, and could drink from neither breast nor bottle; consequently, for a considerable time, it had to be fed by spoon only. After three or four months, however, contrary to all expectation, it gradually improved, and recovered wonderfully, till the 22d of October 1872, when, after a day or two of fretting from teething, it suddenly sank from symptoms which, according

to Dr Affleck, who was kind enough, in my unavoidable absence, to see the child for me, were of such a nature as to establish a diagnosis of acute meningeal effusion. I had not seen the child for about six weeks previously, and then it looked surprisingly well, though the illness had left it blind of one eye, apparently the result of severe iritis.

But now comes the point of most importance in the narrative, and it is connected with the grandmother of the child. She is aged 59, and previously to nursing the child last summer was healthy and well. She was exceedingly attentive to the infant during its illness, and, besides washing it frequently, was in the habit occasionally, when feeding it with the bottle after it was able to suck through the tube, of putting the tube into her own mouth, before and after passing it into the child's, to test whether the tube was pervious and working easily. During the month of June 1872 she began to suffer from sore throat and to feel altogether very ill. Early in July she asked me to see her, when I found her weak and cachectic-looking, as well as suffering from sore throat, and covered all over with an eruption distinctly syphilitic in character. For this I treated her *secundum artem*, and saw her in all only three times. During my absence from town in the end of August she was seized with acute syphilitic iritis, for which, as she did not find me at home, she applied for relief at the Eye Dispensary in Cambridge Street, where she was treated by Mr Benjamin Bell.

From an inspection of the prescriptions furnished her by Mr Bell, and also from inquiry made of the patient herself this forenoon (8th January 1873), I am perfectly satisfied that Mr Bell looked upon the iritis as specific, though I have failed, in consequence of urgent duty otherwise, to ascertain from him personally his views of the case. She is at present very much better in health and spirits than when I last saw her in August of last year.

The sore throat is now quite gone, and so is the eruption, with the single exception of some irregularly annular patches upon the left forearm. Her appetite has returned. The iritis, which affected the left eye, has disappeared, but has left a fixed, small, though regular pupil. The eye, according to the patient's statements, has, however, been gradually improving in strength, and the vision with it is wonderfully good, though compared with that of the sound eye it is weak.

On strict examination, so as to make certain of the facts for this paper, I found that the patient had never perceived any ulcer on her mouth, her lips, her arms, or any other part of her person, previously to the supervention of sore throat. She expressed herself perfectly certain that this, combined with loss of strength, was the first evidence of her illness. She states, however, that her lower lip frequently chaps, and that she remembers, during the time the baby was ill, that it was in this condition.

This case seems to me to need very few remarks. It bears unquestionable evidence on a point, which is, however, so well established as to require little support from any fresh cases, and that is, that it is exceedingly dangerous for a man to enter into the marriage relationship within a period short of two or more years after having contracted constitutional syphilis, however well such an infected person may appear to be. Thus Mr D., apparently without his knowledge of the fact, became syphilitic about one and a half years before marriage. His wife, immediately after falling with child, became constitutionally syphilitic also, and gave birth to a dead child. She seemed to get quite well, yet the next pregnancy only resulted in the birth of a child which, though born apparently healthy, became soon the victim of syphilitic disease in all its disgusting and malignant intensity.

But, in addition to this, it does appear to me to bear very important and irrefragable evidence upon the disputed point as to whether and how far secondary syphilis is communicable. Authors, from Hunter downwards, have been divided on this point; and I must confess that, trusting to the accuracy of the views held on this matter by Hunter and Ricord, I did not sufficiently warn the attendants of this child to guard against infection from the mucous discharges. Lancereaux and a host of other authorities, I find, agree in affirming strongly the opposite doctrine—viz., that constitutional syphilis may be transmitted by means of the secretions from subjects suffering from secondary syphilis. This case, I hold, proves beyond a doubt the correctness of the opinions of the latter class of authorities.

It seems likewise, according to Lancereaux, uncommon for true hard chancre to be developed as a result of infection from syphilis in the secondary stage. It appears that one or other of the forms of the primary disease must result, but that the papule or the chancrous erosion are the forms of primary syphilis that commonly manifest themselves from infection through secondaries. That in the present case there was no true hard chancre developed is evident; but that abundance of opportunity for the existence of either the papule or the chancrous erosion unobserved in the mouth or throat of the patient, is to my mind perfectly evident. I rather think the chap on the patient's lip must have been of the nature of a chancrous erosion.

Feeling convinced that a question of this kind, involving the risk of health and comfort to so many innocent people, was not of a theoretical kind which can be held or dropped without harm resulting, I have been led to report this case from its very positive bearings. I feel very strongly that all of us who have to deal with such unfortunate cases ought to look upon them as decidedly infectious, and not only to interdict such children from being put to the breasts of healthy nurses, but to enjoin the greatest care to keep the discharges from the mucous membranes of such children as are

brought up by hand away from the mucous passages or the broken surfaces of healthy persons. I regret exceedingly that my pathological belief prevented me, in this case, from giving sufficiently strict injunctions to the attendants. I did not believe there was serious danger of infection, and, moreover, did not wish to be too explicit in my injunctions, in case I should create needless suspicion in the minds of Mr D.'s friends as to the cause of the child's illness. Feeling perfectly convinced from this sad case that secondaries are transmissible, I mean in future to be more guarded and decided.

ARTICLE V.—*Puerperal Tetanus*. By JOHN HADDON, M.A.,
M.D., Eccles, Manchester.

TETANUS, in any of its forms, is a serious disease; but puerperal tetanus is, most of all, to be dreaded, since the parent's life at that time is doubly precious. So little is known either as to the etiology, pathology, or successful treatment of this disease, that it would seem the duty of every practitioner to record any case that may come under his notice. It was my misfortune to witness this disease in one in whom I had a deep interest; and, by the kindness of the medical attendant (Dr Ransome), I am enabled to furnish a report of the case.

Mrs —, aged 39, had had eight children, and was usually in good health. She did not nurse her children. About two years ago, however, when six months advanced in pregnancy with her last child but one, she had an attack of partial aphasia, with slight paralysis, and some twitching of the muscles of the right side of the face. There was no general hemiplegia, and the attack soon passed off under the use of ammonia and bromide of potassium. There was at that time no albuminuria. The labour took place at the natural time, and without any drawback. During pregnancy with her last child, she was much harassed with sickness in her household, but no untoward sign showed itself. Labour began on 11th March 1873. The first stage lasted twelve hours, much longer than usual, her confinements generally being very rapid. There were only three or four bearing-down pains before the birth of the child. The placenta came away in about twenty minutes, and was expelled by the help of pressure on the uterus, without any traction. It was soft, but apparently entire. There was very little hæmorrhage, and she did well until 13th March.

On the morning of that day she had a rigor lasting half an hour, and there was much tenderness over the hypogastrium. She herself attributed her shivering to the action of the castor-oil she had taken that morning, and which, she affirmed, often produced that effect. Turpentine stupes were applied to the abdomen, and injections into the vagina of Condyl's fluid and water were ordered.

In the evening she felt much better, and the tenderness over the hypogastrum had disappeared.

On 14th March, difficulty of speech was noted, chiefly with the more complex words, but gradually increasing in extent. There was no pain or tenderness on pressure over the abdomen. The lochia were normally abundant, and free from odour. At 9 A.M., temperature, 104° ; pulse, 120. At 9 P.M., temp. 105° ; pulse, 120. The uterus, as felt through the abdominal walls, was large and rather soft, but entirely free from tenderness. Bromide of potassium and ammoniated tincture of valerian were ordered in half-drachm doses.

On the 15th, a small offensive clot came away, but Condy being used, the lochia were not afterwards offensive. At 10 A.M., temp. 103° ; pulse, 120. At 9 P.M., temp. 102° ; pulse, 120.

On the 16th, the difficulty of speech increased until she could hardly say the shortest word, and when the attempt was made there was much twitching of the facial muscles. At 10 A.M., temp. 103° ; pulse, 130. At 9 P.M., temp. 103.2° ; pulse, 130.

On the 17th, in early morning, she had a tetanic spasm of the jaw and trunk, and at 7 A.M. the pulse was 160, and the temp. 106° . She could not speak, but seemed conscious. A vaginal examination discovered the uterus large and flabby, easily allowing the entrance of the hand. Upon the posterior and right side of the uterus some foreign substance was detected. It felt, in consistence, similar to the uterus itself, seemed incorporated in its walls, and, after several attempts, it was evident that its removal could not be accomplished without injury to the uterine substance. At 11 A.M., the pulse was 130; temp. 104° . The tetanic state continued, with exacerbations at intervals. She lay with her eyes shut, and could not speak. At 5 P.M., the pulse was 140; temp. 102.6° . At 6.30 P.M., she lay with her arms flexed rigidly over the chest, and the fingers clenched. Her eyes were shut, and her teeth and lips firmly closed. Her breathing was quick and loud. The pupils, which were somewhat dilated, answered to light. The pulse was 140, and the temperature 102.6° . She seemed to be conscious, since, on asking if she knew me, she winked hard, meaning, apparently, that she did, though she could not say so. The abdomen was not tympanitic, and the uterus could be felt large and soft through the parietes. On vaginal examination, under the influence of chloroform, the parts were found as in the morning. The discharge was not at all offensive. The advisability of attempting to remove the foreign substance was discussed; but, as it seemed, on further careful examination, to be so closely incorporated with the uterus, and considering the state of the patient, it was decided not to interfere. The chloroform allayed the spasm, but only to return as its influence passed off. Beef-tea enemata were being administered every two hours. The urine, which required to be drawn off by catheter, was albuminous. At 6.45, thirty minims of cannabis Indica were given by the mouth, as she could swallow a little. This

dose was repeated every two hours, but without any benefit. At 8 P.M., the spasm was so violent that chloroform was administered. As before, all the muscles relaxed, and she breathed quietly while under its influence, but so soon as that influence ceased, the spasm returned so severely that the chloroform administration required to be almost continuous. About 1 A.M., being from under the influence of chloroform, and in violent spasm, a solution of thirty grains of chloral was given per rectum. About 1.20 A.M., the spasm began to relax, the pulse to fail, and she for the first time attempted to articulate, but quite unconsciously. The spasm did not return, and in a few minutes she peacefully breathed her last.

The pulse could scarcely be counted some hours before death; and the following figures show the state of the temperature:—

At 9	P.M.,	.	.	.	temperature,	106°
9.15	"	.	.	.	"	106.4°
9.35	"	.	.	.	"	106.4°
10	"	.	.	.	"	106.2°
10.30	"	.	.	.	"	106°
12	"	.	.	.	"	106.4°
12.30	A.M.,	.	.	.	"	106.2°
1.20	"	.	.	.	"	108°

A post-mortem could not be obtained. This case seems to have been one of no ordinary character. At first, the symptoms, as described, point to some affection of the nervous system not observed in tetanus, but commonly seen in cases of paralysis. In the latter stage of the disease, too, there were symptoms not usually observed in uncomplicated tetanus, which gives interest to the case in so far as regards the particular part of the nervous system primarily affected. In considering puerperal tetanus, it would be well to keep in mind the disease called "tetany," often seen in nursing mothers, and bearing considerable resemblance to the more dangerous affection.

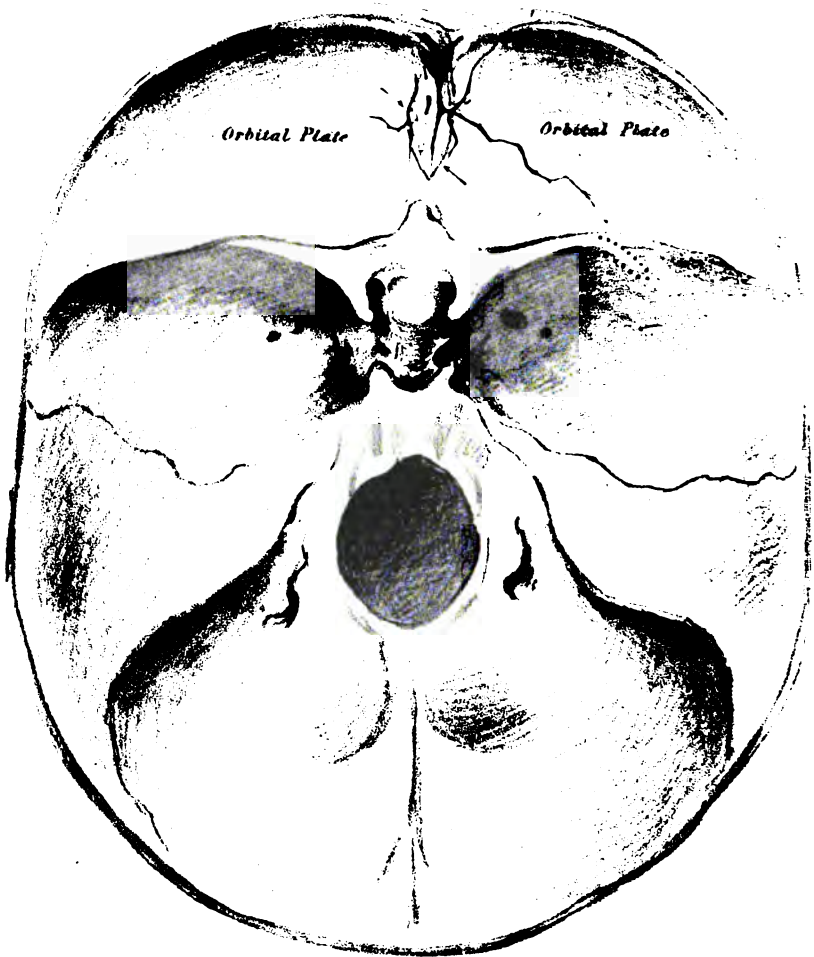
ARTICLE VI.—*Internal Fracture of the Skull, with Puncture of the Brain.* By G. R. GILRUTH, L.R.C.S.E.

(Read before the Medico-Chirurgical Society of Edinburgh, 5th March.)

INJURIES of the head, owing to their frequency, and from their effects upon the mental faculties, are always interesting, and demand attention. Not to say that such injuries are more fatal than severe injuries to such organs as the heart, liver, and kidneys; but injuries to these latter organs occur more rarely, and, being less amenable to treatment, usually prove more rapidly fatal.

The brain, it is known, may be severely injured, and yet life, and even consciousness, may still exist. Small quantities of brain matter have been observed on the dressings and in the discharges after penetrating wounds of the head; and withal, recovery has

TRANSVERSE SECTION OF SKULL—FRACTURE OF THE SKULL &C.



—→ Points to Orbiform Plate
— Lines of Fracture

..... The dotted red line shows the upward bent and dislocated process

taken place. Foreign bodies have become encysted; and mention is made of a man who lived for six months with an abscess in his brain. In the case which I am about to report, the brain was deeply punctured by a piece of displaced bone, and although embedded in the cerebral substance, the child lived for six days, and at times appeared conscious.

The great danger to life seems to depend not so much upon the wounding of the brain as upon the suppuration which follows the inflammatory action depending on the injury.

CASE I.—*6th September 1869.*—Ann R., a child of three years, fell into an area, a depth of about seven feet. An hour after the accident I saw her, and found her unconscious. The face was pale, the forehead bruised, but no wound; the right eyelid closed and blackened; there was bleeding from the nose, the breathing stertorous, and the pulse 62. The left eye was fixed, and its pupil insensible and dilated. There was no depression of bone anywhere; but on passing a probe up the right nostril, it grated distinctly against bare bone.

7th.—Appeared conscious, took notice of a dog in the room; partook of food; pupil fixedly dilated. In the evening, became feverish; pulse, 120.

8th.—Pulse, 120; pupil contracted; drowsy; features sharp. Towards night, became convulsed; pulse, 80; signs of collapse.

9th.—Convulsive action of the lower jaw, producing a clattering sound from the teeth. Pupils dilated and immovable.

10th.—Facial muscles of the right side paralyzed. Sensible at intervals.

11th.—Comatose. Features drawn to the left side. A yellowish mucus exuded from the mouth. Eyes inverted.

12th.—A pale bluish discoloration of the face; eyes prominent and lustreless. Died that night.

With the assistance of a medical student and J. H. M'Closkey, Esq., now the Colonial Surgeon, Labuan, Borneo, I opened the head.¹

The membranes bore traces of inflammation, and in some places there was pus.

The cribriform plate of the ethmoid was fractured on both sides of the crista galli; a fracture also ran forwards and backwards through the right orbital plate of the frontal bone. The external end of the small wing on the right side of the sphenoid was dislocated backwards and slightly upwards. On removing the brain, a small punctured wound, corresponding to the situation of the dislo-

¹ The diagram represents a horizontal section of the skull.

The red lines show the lines of fracture.

The dotted red line, the dislocated process.

I am greatly indebted to Dr Handyside for this diagram, taken from a rough sketch of my own.

cated process, was found on the under surface of the right anterior lobe of the cerebrum.

The paralysis of the face, and this wound, which was in a suppurating condition, were both on the same side of the body: a rare occurrence, but one which has been observed before.

Diagnosis.—The diagnosis from the first was that of compression of the brain.

The pulse at certain times can afford much information, but it is only, as far as symptoms go, by watching the state of the pupil, that a correct diagnosis and prognosis can be formed.

Fixed dilatation of the pupil is a valuable indication of structural injury of the brain, and indicating no doubt a loss of nervous power.

In concussion, the pupil may be either dilated or contracted, but seldom long insensible; in the early stages of inflammation of the brain or its membranes, it is usually contracted; but in the suppurative stage of the inflammation, and in compression, it is always insensible and dilated.

The insensibility immediately after the accident might be ascribed to either the concussion or to the wounding of the brain; the coma, however, depended on the suppuration.

Prognosis.—It must always be doubtful; for a slight injury may be followed by fatal inflammation. In head affections, a marked improvement often takes place in the condition of the patient, but frequently to prove transitory and deceptive.

Treatment.—Cold to the head, leeching, bleeding from the arm, etc., were employed.

As for the use of the trephine in depressed fractures, its application appears to be the most rational form of treatment, when there is a piece of depressed bone embedded in the brain substance; for no amelioration in the symptoms can be expected until the exciting cause has been removed.

Scarcely a century ago, it was customary to employ the trepan in simple fracture; a ridiculous procedure, as modern pathology has shown, except when there is evidence of pus, or extravasated blood under the cranial bones. In the case of this child, however, the seat of injury was beyond the reach of the trephine; and there was neither external depression nor puffy tumour to indicate its use.

Perhaps it is advisable not to be too hasty in employing the trephine where children are concerned; for in them the cranial bones are soft, and the brain seems to have the power of accommodating itself to some alteration in the form of the skull.

Quesnay, an eminent French surgeon, in his valuable memoir "On the Use of the Trepan in Doubtful Cases," mentions the case of a girl of fourteen, who received a blow on the head, causing a depression of the right parietal bone, followed by symptoms of compression. A surgeon wished to use the trepan, but the mother of the girl refused to allow it.

The stupor and delirium kept the patient in a state of imbecility

for a period of three months. The bone, however, gradually regained its level, and all the bad symptoms disappeared.

In the event of suppuration, some great authorities have contended, that in order to give vent to pus, not only may the dura mater be opened with safety, but that an incision may be made into the brain when an abscess forms within its substance.

The value of post-mortem examination is here well exemplified; for without it the exact nature of the injury could never have been ascertained.

This seems, too, to be a form of fracture very likely to occur in children, owing to the tendency they have in falling to pitch on the forehead.

CASE II.—Some time ago, I had an opportunity of seeing a case in the practice of another medical gentleman, similar almost in every respect to the one just related. The injury, occasioned by a fall, was confined to the frontal region. The child lived for some days; but after death a post-mortem was not obtained.

A case came under my notice lately, and although the child fell from a height as great as in the first case, yet the only bad effect produced was concussion.

7th March 1872.—Called to see the child of Mr H., aged eighteen months, who had fallen from a first-flat window into a very deep area upon his head.

He was unconscious, had several convulsive attacks, and at times there was a great tendency to sinking.

The forehead was slightly bruised, the nose bled, but the probe failed to detect bare bone in the roof of the nostril. A sinapism was applied to the epigastrium; and with other remedies had the effect of restoring the child a little. Next day, greatly improved. Recovery was rapid. Dr Ziegler, the family physician, also saw the case.

This was simply a case of concussion, for there was no positive sign of fracture; but concussion or shock sometimes kills the individual by its sudden effect, causing in all probability an immediate paralysis of that part of the nervous system connected with circulation and respiration.

A good illustration of death from concussion may be taken from the ordinary mode of killing a tame rabbit, by striking it with the edge of the hand on the back of the head.

On dissecting an animal so killed, I could find neither fracture nor dislocation in the cranial or cervical region.

The upper part of the cord was congested, no effusion into the spinal canal, but there was a quantity of extravasated blood external to it. While speaking of concussion, I may mention the following fact:—In a railway accident which occurred in 1870, it was reported that those persons who were asleep at the time, escaped without shock.

ARTICLE VII.—*On Septicæmia: A Record of Investigations made by Dr Gesualdo Clementi and Dr George Thin, in the Institute for Experimental Pathology, Vienna.* By GEORGE THIN, M.D.

IN Sept. 1872, M. Davaine presented to the Academy of Medicine in Paris a report on a series of experiments, which showed that, by injecting blood in certain stages of putrefaction into the cellular tissue of several of the mammifera, and especially of the rabbit, he produced a disease capable of being transmitted to others in an unlimited series by inoculation of the unputrefied blood of the animals affected. His theory as to the nature of the disease so produced is expressed in his selection of the term "septicæmia" for its designation,—a word for whose discovery M. Piorry undisputedly claims priority.

Davaine disclaims absolute originality in his experiments. Magendie, he tells us, recognised the transmissibility of the disease, as well as an increase in the virulence of the poison when transmitted. It was in later times (1866) demonstrated by MM. Coze and Feltz, and, as regards guineapigs, by Davaine himself in 1869. MM. Coze and Feltz recognised also the increased virulence by transmission; but to Davaine belongs the credit of having, by a very great number of experiments, persevered in during a period of several years, and by bringing into prominence the infinitesimal doses in which the poison retains its activity and certainty, drawn attention in a marked manner to the importance of the questions that are involved in the study of the septicæmia of the rabbit.

Davaine began a series of experiments with ox-blood which had been allowed to putrefy during ten days in July, and made his dilutions by taking the drop as unity. A number of rabbits and guineapigs were inoculated with varying doses of the blood so putrefied. These inoculations with putrefied blood producing a fatal disease, blood from the animals that died was injected (unputrefied) into others. These, again, succumbing to the effects of this inoculation, and their blood being used successfully for further inoculations, he created a series of successive generations of the poison, which he designated as first, second, third, and so on, in their order. He thus acquired material for comparison of the effects of the injection of putrefied blood, and of the unputrefied blood obtained from the generations created. The latter is designated by Davaine as septicæmic blood. In this paper, I shall designate it as transmitted blood.

Of 72 guineapigs into whose subcutaneous cellular tissue he injected doses of putrefied blood varying from 1 to 10 drops, 43 survived, and 25 died. He did not succeed in killing a guineapig with a smaller dose than $\frac{1}{16}$ th of a drop.

Of 48 rabbits, inoculated with a dose varying from 1 to 16 drops, 22 survived, and 26 died. Of 9 others, in which the dose injected was less than a drop, none died with a smaller dose than $\frac{1}{1000}$ th of a drop.

He found that a guineapig rarely died when the dose was under $\frac{1}{16}$ th,

or a rabbit when under $\frac{1}{125}$ th of a drop. But when he injected transmitted blood, he found that death resulted from very small doses, whose minuteness contrasts with the limit of fatality in putrefied blood.

He produced a series of generations from a rabbit that died in forty hours, after having had an injection of 10 drops of putrefied blood, and gives the following extracts from his protocol, showing part of the results:—

First Generation.—From the heart of this rabbit, 12 hours after death, doses of 1, 2, 3, and 4 drops of blood were injected respectively into 4 rabbits. They all died in the same night, between 30 and 40 hours after the inoculation.

Fifth Generation.—3 rabbits, with doses of 1, $\frac{1}{16}$ th, and $\frac{1}{125}$ th of a drop, from the heart of a rabbit of the fourth generation. 2 died in 14 hours, the third in 20 hours.

Tenth Generation.—3 rabbits were inoculated with the blood of a rabbit of the ninth generation an hour after its death, with doses of 1, $\frac{1}{10,000}$ th, and $\frac{1}{20,000}$ th of a drop respectively. The first died the following night, the second 15 hours, and the third about 35 hours after the inoculation.

Fifteenth Generation.—3 rabbits were inoculated with $\frac{1}{20,000}$ th, $\frac{1}{30,000}$ th, and $\frac{1}{40,000}$ th of a drop of blood. They died, all three, in the interval between 20 and 40 hours afterwards.

Twentieth Generation.—The blood of a rabbit, dead one hour, was injected in doses of $\frac{1}{500,000}$ th, $\frac{1}{1,000,000}$ th, and $\frac{1}{100,000,000}$ th of a drop. The three rabbits which received these infinitesimal quantities of blood died, the first and third in 35 hours, the second in 21 hours.

I pass on to the *Twenty-fourth Generation.*—5 rabbits were inoculated with the blood of a rabbit dead from the hundred-millionth of a drop. The first received a hundred-millionth, the second a billionth, the third a ten-billionth, the fourth a hundred-billionth, and the fifth a trillionth of a drop of blood, from the above-mentioned rabbit. All these animals died in less than 24 hours.

Finally, in the *Twenty-fifth Generation*, 4 rabbits received a trillionth, a ten-trillionth, a hundred-trillionth, and a quadrillionth of a drop of blood from a rabbit belonging to the preceding series, and dead of the trillionth of a drop. Only one of the four died—that which received a ten-trillionth of a drop of blood.

Davaine infers from these experiments that transmitted blood found its limit of effect in the trillionth part of a drop, whereas, for putrefied blood, he had found it to be the two-thousandth.

MM. Coze and Feltz had recognised that there is a difference as regards the rapidity of death after inoculation with putrefied and transmitted blood; and this is confirmed by Davaine.

Of 5 rabbits injected with putrefied blood (one of which formed the source of his transmitted generations), 1 died between 30 and 40 hours; 1 in 16 days; 1 in 9 days; 1 in 26 days; and 1 in 5 days. Of 69 injected with transmitted blood, 67 died within 40 hours.

It held good, both in putrefied and transmitted blood, that the length of time that elapsed between the inoculation and the death of the animal bore no relation to the dose injected.

He further relates facts showing that transmitted blood acquires its extreme virulence from the first generation; that putrefaction destroys the virulence of transmitted blood; and that guineapigs are not so sensitive relatively to the dose as rabbits are.

He found that a rat survived a dose of a hundred-thousandth and a ten-thousandth of a drop. A mouse died with a hundred-thousandth. Birds survived the inoculation.

He gives some interesting facts regarding the comparative power of resistance of different animals to infected blood from different sources. Although, in transmitted blood whose original source was putrefied ox-blood, guineapigs survived doses that would have been certainly fatal in rabbits, the contrary was the case in injections made with blood taken from a cadaver in which the cause of death was *pustule maligne*. In the latter case, a guineapig succumbed to the dose of a millionth of a drop, whilst the thousandth of a drop was found to be the limit of a fatal dose for a rabbit.

He further found that putrefied blood loses its virulence after the putrefaction has passed a certain stage.

Davaine has a theory to explain the above facts. He believes it to be probable that transmitted blood loses its virulence when it putrefies, because the products of putrefaction destroy the active principle present in the blood; and that it acquires such a great intensity in the living animal because the substances that would, if present in quantity in the blood, destroy its power, are eliminated by the excretory organs as quickly as they are formed. He epitomizes this theory when he states that the inoculation of septicæmic blood into the living animal produces a putrefaction of the blood while the animal still lives.

Fresh blood, inoculated with a minute quantity of putrefied blood, and kept at a temperature of 35° to 39° Centigrade for 14 hours, was fatal in doses of a fifty-thousandth and a millionth; mixed with charcoal and carbonate of lead, and similarly treated, it killed in equally minute doses;—from which Davaine concludes that blood, when removed from the animal, can acquire properties as virulent as it can acquire in the living body; and that the virus of septicæmia consists in the putrefaction of a living animal. He expresses, at the same time, his belief that the virus of septicæmia is not the only virus in putrefied matter.

He accepts the doctrine of Pasteur, that putrefaction is a fermentation determined by the presence of bacteriæ, which he believes to be present in septicæmic blood, and to be the cause of the alleged putrefaction.

He believes all animals to be liable to septicæmia, the differences in the rapidity and intensity of the disease produced depending on the putrescibility of the animal and the surrounding temperature.

The rabbit especially he finds so liable to be affected, that he

would make the result of inoculation on this animal the test of a fluid being septicæmic. In his experiments, of several hundred rabbits inoculated with doses extending in dilution to a trillionth, only one did not die, and he attributes its recovery to the formation of an abscess at the point where the inoculation was performed.

Using the rabbit as a test, he injected minute doses of blood, taken from patients ill of typhoid fever. The results he obtained are so important, that I have put them together in the following Table:—

No.	Description of Patient who furnished the Blood.	Day of Fever at time of Inoculation.	Dose in Drops (diluted in water).	Time from the Inoculation to the Death of the Rabbit.	Remarks.
1	Male, æt. 45, recovered	15th	100	A month.	A rabbit inoculated with a dose of a millionth of a drop of blood, taken from No. 1 immediately after death, died in 46 hours.
2	Male, æt. 15, recovered	15th	1000	11 days.	
3	...		1000000	15 "	
4	Male, æt. 18	18th	1000	10 "	
5	...		1000000	10 "	
6	...	18th	1000	10 "	
7	...		1000000	10 "	
8	...	20th	1000	10 "	
9	Male, æt. 23, recovered		1000000	18 "	
10	...	25th	1000000	24 "	Patient was improving. Patients' symptoms aggravated.
11	...	27th	1000	10 "	
12	...		1000000	17 "	
13	...	7th	1000	10 "	
14	...		1000000	17 "	
15	...	9th	1000	7 "	
16	...		1000000	34 "	
17	Male, æt. 16, recovered	15th	1000	1 "	
18	...		1000000	11 "	
19	...	37th	Dose not stated	...	At the time Davaine read the report from which these figures are extracted, 17 days after the injection, these rabbits were both healthy, and the patient in full convalescence.
20	...			...	

No distinctive lesions were found in the animals dead from direct inoculation with typhoid blood; and it will be observed that in the second generation the time that elapsed between the inoculation and death of the animal coincides with that in cases of death from inoculation with transmitted blood which has putrefied blood as its source.

The communication by Davaine of the results of his investigations and especially the statements as to the power of the infinitesimal doses, led to their being repeated by other experimenters,

have since at different times read reports on their experiments before the Paris Academy.

M. Behier experimented with blood heated in accordance with Davaine's instructions for fourteen hours, confirmed the fatal results that followed its injection and that of the transmitted blood of which it was the source, and drew attention more particularly to the lesions found in the cadavera of the dead animals. Davaine had stated that there is no special lesion distinctive of septicæmia, but in subsequent discussions admitted the occasional occurrence of the post-mortem appearances signalized by Behier. These were tumefaction of the jaw and commencement of lobular pneumonia. Behier further examined the serous fluid with which the maxillary region was infiltrated, and found irregular white corpuscles and very numerous "*bâtonnets animés*." In blood taken from the heart he also notes "*corpuscles animés*." In blood taken from a live rabbit affected with the septicæmic poison, he found "*corpuscles animés*" in great number.

He inoculated a rabbit with ten drops of blood taken from a patient ill of typhoid fever, and it died in less than sixty-four hours. Another rabbit inoculated from this one after its death died two days after inoculation. The patient from whom the blood had been taken died, and blood taken from the cadaver was injected in the doses of ten drops of a twenty-thousandth dilution, and one drop of pure blood into two rabbits respectively. Both animals survived.

M. Vulpian confirmed the activity of transmitted septicæmic blood, finding it fatal in doses of a thousandth and a millionth, but not in a billionth. In the dilutions to a millionth he found "*granulations*" and bacteriæ under the microscope. In blood taken from the ear in rabbits sickened after inoculation he found the white corpuscles increased in number, and that the plasma contained "*bacteriæ très courtes et très grêles, et granulations mobiles ou immobiles*." The post-mortem appearances he found to be—*1st*, Infiltration at the point of inoculation; *2dly*, An altered condition of the blood ("*dissolution de ce liquide, altérations des globules, myriade de fines granulations et de bâtonnets dans le serum ou le plasma*"); *3dly*, Varying alterations in the thoracic and abdominal organs.

He believes that for the production of septicæmia the presence of bacteriæ in the blood is necessary. For him, "*la septicémie expérimentale du lapin paraît être une sorte d'affection parasitaire interne*," which he thinks ought to be called "*bactériémie*."

He failed to produce septicæmia in rabbits by the injection of blood from patients ill of typhoid fever.

Experiments made by M. Bouley on horses, dogs, and rabbits showed that they were all liable to be affected by septicæmia, and that the blood of one species fatally inoculated another.

In regard to the nature of the virulent element present in the blood, the following experiment was made by M. Onimus:—

Blood from the ox or swine, or human blood from a patient suffering from typhoid fever, is placed in dialysis paper, and this paper is then placed in a vessel containing distilled water, and the whole submitted to a temperature of about 35° Centigrade, according to the indications given by Davaine. After fourteen hours, the distilled water becomes troubled to lactescence, and, examined by the microscope, is found to contain a prodigious quantity of bacteriæ and vibrios, identical in form with those contained in the blood enclosed in the dialysis paper. This being established, Onimus injected one drop of the putrefied blood contained in the dialysis paper into several rabbits; whilst several drops of the external fluid, in which were myriads of bacteriæ, were injected into other rabbits. All the animals which received a drop of putrefied blood died in a short time; all those which received the water with the bacteriæ survived. Similar experiments made with the blood of rabbits that died from the injection of putrid blood he found gave similar results. Relying on these experiments, Onimus maintained, in opposition to Davaine, that the presence of bacteriæ is not the essential element in the production of septicæmia.

The communication of his experiments to the Académie de Médecine by Davaine induced Dr Gesualdo Clementi and myself to repeat them—the resources of the Institute for Experimental Pathology in Vienna, and his invaluable advice and assistance, being put at our disposal by the kindness of Professor Stricker. An account of our experiments appears in the forthcoming number of the “*Med. Jahrbuch*”, in Vienna; but I have written the following account of them in English for the benefit of those who, although interested in the subject, may not have access to what we have published jointly in Germany.

Our experiments were begun in the end of February, and were interrupted in the beginning of May. The results published by the French observers seem at first sight so improbable in themselves that, notwithstanding the high authority that vouched for their accuracy, the publication of our protocol, even although it to a great extent is only confirmatory of the Paris experiments, has a utility of its own. And although we have not contributed much, we have still done something towards elucidating the problems connected with the question.

The rabbits which we used for our experiments were housed in roomy hutches constructed of boards and wire-netting. Although in the vicinity of the dead-house of the hospital, they were free from all influences that might have proceeded from this proximity, the hutches being supported by a wall that is separated from the buildings of the institution, and having a free exposure to a garden several hundred square yards in extent.

Each animal, as soon as it was inoculated, had a wire collar fastened round its neck, on which was hung a tin plate bearing a stamped number; and at each inoculation the number on the plate, the dose, and kind of blood inoculated, were entered in the

protocol. The healthy and inoculated animals were associated promiscuously in the hutches.

The inoculations were invariably made by injections into the subcutaneous cellular tissue of the neck. The instruments used were those which are ordinarily employed for subcutaneous injection. We provided ourselves with a number of these syringes, so that in most cases injections of healthy blood were made with syringes that had not come in contact with diseased blood.

The dilutions were made after the manner of the homœopathists. The capacity of the syringe we used is 0·80 cubic centimetre. It is marked with decimal divisions, and one of these divisions was taken as unity in our dilutions. In all doses which did not exceed 0·08 gramme, the mass of injected fluid was 0·08. The dose 0·08, the smallest we could accurately measure with the syringe, represents the corresponding quantity of undiluted blood; 0·008 represents blood mixed with ten times, 0·0008 with a hundred times, its volume of water.

The blood was sometimes taken from the animals when they were mortally sick, and sometimes after they had died from inoculation.

As we did not occupy ourselves specially with the symptoms of the disease produced, no pretension is made to give an accurate and exhaustive history of it, further than to state briefly that the animals gave up eating, stood motionless in their stalls, were affected with dyspnoea, leant to one side in a very characteristic manner, and towards the end were frequently convulsed, especially if they were moved. The swelling of the muzzle which was sometimes present was inconstant.

The blood of the affected animals was repeatedly examined under the microscope at different stages of the malady. The specimen examined was usually obtained by inserting a fine glass tube into the jugular vein. Rounded or irregularly-shaped granular elements, varying in size, were present in great number; the white corpuscles were strikingly large, and were present in a greater proportion than in normal blood. The blood frequently coagulated under the microscope with great rapidity, so that the known plaques composed of threads and granules were visible. Vibrios and bacteriæ were not once seen in blood taken from the living animal, even when it was *in articulo mortis*; whilst they were found abundant in every specimen of putrefied blood we put under the microscope.

The number of animals on which experiments were made by injecting healthy, putrefied, and transmitted blood amounted to 123 rabbits, 6 guineapigs, and 2 dogs.

The first dog had 0·8 cubic centimetre of putrefied blood injected under the skin. The injected part swelled and became tender, the thermometer indicated a moderate degree of fever, and the animal died the sixth day after the injection. Nothing noteworthy was found on a post-mortem section.

Blood was taken from the cadaver of this dog, and 0·8 cubic centimetre was injected into another dog. The animal had fever during the following day, began to eat on the second day, and on the third was quite recovered. Twenty days later, the same dog received an injection of 1·6 cubic centimetres of blood from a dying rabbit (eighth generation of transmitted blood). The same symptoms followed the injection as in the former instance, but it soon recovered, and after two months was employed for other purposes. The results of our experiments on rabbits are given in the following Tables:—

TABLE I., *giving a General View of the Experiments on Rabbits.*

Kinds of Blood Injected.		Numbers	Died.	Survived.	Total.	Died.	Survived.
Healthy	{ Dialyzed.....	2	0	2	27	4	23
	{ Pure.....	25	4	21			
Putrefied.....		23	11	12	23	11	12
Transmitted	{ Pure.....	49	37	12	73	54	19
	{ Boiled	11	9	2			
	{ Dialyzed.....	8	8	0			
	{ Treated with alcohol	3	0	3			
	{ Distilled	2	0	2			
					123	69	54

TABLE II.—*Inoculation with the Blood of Healthy Animals.*

Dose in Cubic Centim.	Number of Animals inoculated.	Died after Inoculation.	Survived, and employed for other Purposes.
0·4	4	0	On the — day. On the — day. On the — day. On the — day.
0·16	8	On the 5th day On the 3d day On the 9th day On the 20th day	On the 20th day. On the 20th day. On the 28th day. On the 39th day.
0·08	13	0	Began to be used for other purposes from the 14th day onwards.
	25	4	21

Table I. shows that the mortality amongst the animals inoculated with transmitted blood was at the rate of 73·9; with putrefied blood, 47·8; and with healthy blood, 15 per cent.

As during the time that our experiments were being made not

week passed without one or two deaths occurring amongst the rabbits kept in store, and assumed to be healthy, the case of death that occurred on the 20th day after the inoculation can easily be considered as possibly due to other causes. In two of the three others that in the Healthy Blood series received a dose of 0.16 cubic centimetre, a syringe was used that had been previously employed for the injection of putrefied blood. We, however, believed it to be prudent to accept these cases as they occurred, and to admit that the mortality even with healthy blood is 15 per cent.

TABLE III.—*Inoculation with Putrefied Blood.*

Number.	Number of Animals.	Dose in Cubic Centimetres.	Died after Inoculation.		Survived, and employed for other Purposes.	
			Number.		Number.	
1	...	...	...	On 2d day.	...	
2	...	...	...	" 2d "	...	
3	...	...	...	" 2d "	...	
4	7	0.4	5	" 2d "	2	
5	...	...	...	" 3d "	...	
6	...	...	...	...	...	On 19th day.
7	...	...	...	...	...	" 14th "
8	1	0.24	1	" 11th "	...	
9	...	...	...	" 7th "	...	
10	3	0.16	1	...	2	" 23d "
11	...	...	...	...	...	" 24th "
12	1	0.08	...	...	1	" 19th "
13	...	...	...	" 2d "	...	
14	4	0.00008	2	...	2	" 25th "
15	...	...	...	" 12th "	...	
16	...	...	...	...	...	" 19th "
17	1	0.000008	...	...	1	" 25th "
18	...	...	...	...	...	" 25th "
19	4	0.0000008	1	...	3	" 25th "
20	...	...	...	...	...	" 29th "
21	...	...	...	" 9th "	...	
22	2	0.00000008	2	" 12th "	...	
23	...	...	...	" 3d "	...	
			12		11	

If we compare Table III. with Table II., a striking difference is observable in the results of inoculation with the larger doses. Of 7 animals that received 0.4 cub. cent. of putrefied blood, 5 died; while of 4 that received a like dose of healthy blood, all survived. It is to be remarked, that the two animals which had received doses of 0.00000008 cub. cent. of putrefied blood died, whilst the mortality from larger doses was much smaller. As already observed, in the animals that died after inoculation with transmitted blood, extensive infiltration of the skin and subcutaneous cellular tissue was found at the part where the injection had been made. This infiltration failed in those which had injections of healthy blood, or very small doses of putrefied blood—a circumstance which may leave room for suspicion of infection in certain cases. But this is one on which our experiments are defective.

TABLE IV.—*Inoculation with Transmitted Blood.*

No.	Number of Animals.	Dose in Cubic Centimetres.	Died after Inoculation.		Survived and Employed for other Purposes.		Generation.
1	...	...	No.	...	No.	...	...
2	4	0-16	1	On 4th day.	3	On 20th day.	2
3	...	...	...	...	...	" 20th "	2
4	...	...	...	...	...	" 20th "	2
5	...	...	...	" 3d "	...	...	3
6	...	...	...	" 2d "	...	...	4
7	5	0-08	5	" 3d "	...	...	4
8	...	...	...	" 2d "	...	...	8
9	...	...	...	" 5th "	...	...	9
10	...	...	...	" 4th "	...	...	5
11	...	...	...	" 3d "	...	...	5
12	...	...	...	" 3d "	...	...	5
13	8	0-016	7	" 4th "	1	...	5
14	...	...	...	" 4th "	...	...	2
15	...	...	...	" 2d "	...	...	8
16	...	...	...	" 3d "	...	...	8
17	...	...	...	...	...	" 20th "	2
18	...	...	...	" 3d "	...	...	6
19	4	0-008	4	" 4th "	...	...	6
20	...	...	...	" 2d "	...	...	8
21	...	...	...	" 4th "	...	...	11
22	...	...	...	" 10th "	...	...	3
23	...	...	...	" 3d "	...	...	6
24	6	0-0008	6	" 4th "	...	...	6
25	...	...	...	" 3d "	...	...	7
26	...	...	...	" 4th "	...	...	7
27	...	...	...	" 3d "	...	...	11
28	...	...	...	" 5th "	...	...	7
29	...	...	...	" 6th "	...	...	7
30	6	0-00008	6	" 4th "	...	...	7
31	...	...	...	" 5th "	...	...	7
32	...	...	...	" 5th "	...	...	10
33	...	...	...	" 2d "	...	...	12
34	...	...	...	...	...	" 10th "	...
35	...	...	...	...	...	" 29th "	...
36	...	...	...	" 2d "	...	...	...
37	...	...	...	" 4th "	...	...	...
38	9	0-000008	5	" 4th "	4	...	...
39	...	...	...	...	...	" 6th "	...
40	...	...	...	...	...	" 28th "	...
41	...	...	...	" 2d "	...	...	...
42	...	...	...	" 2d "	...	...	...
43	2	0-00000008	2	" 5th "	...	...	6
44	...	...	...	" 3d "	...	...	6
45	1	0-000000008	...	...	1	" 30th "	9
46	1	0-0000000008	1	" 3d "	...	...	9
47	2	0-00000000008	...	...	2	" 20th "	9
48	...	...	...	...	...	" 20th "	7
49	1	0-000000000008	...	...	1	" 20th "	9
			37		12		

In Table IV., it is noticeable that the animals that survived the injection of transmitted blood were mostly either those that belonged to the generations that formed the starting-point of the series, or to those that received exceedingly minute doses.

After examining this Table, it is impossible not to be struck by the fact that of 29 animals that received doses of 0·08 to 0·00008, only 1 survived, and that this single exception belonged to the second generation; that only one lived to the 10th, one to the 6th, and four to the 5th day after inoculation.

That we had not here to do with deaths from other causes is proved by the fact that the part where the inoculation was effected was, without exception, the seat of infiltration.

As the dose decreases from 0·000008 cub. cent., the rate of mortality begins to decrease in a striking manner; but our experiments were not sufficiently numerous to define the degree of dilution where the poison ceases to take effect.

We thus learned, from our experiments, that the mortality in the animals inoculated with transmitted blood was very much greater than in those inoculated with putrefied blood, and that an approximative dose of 1 decimilligramme of transmitted blood was sufficient to kill a rabbit.

Further, when it is considered that in these experiments transmitted blood to the twelfth generation was used, it becomes evident that the noxious agent must have multiplied in the animals after inoculation. For even if we suppose that the blood alone, and not the other fluids of the animal's body, acted as a diluent of the dose injected, we would have, by the time we came to the twelfth generation, fractions of a gramme in a dose that would have to be written with 50 to 60 ciphers. But our experiments made with doses written with 10 ciphers, gave already negative results.

As, in accordance with accepted definitions, the noxious substances present in putrefied blood must be designated as miasmata, and those conveyed from one animal to another and multiplied in the organism of the animal as contagia, the conclusion follows from our experiments, that a miasm has become transformed into a contagium.

As to the nature of the miasma and the contagium with which we have here to do, we express no opinion, our experiments being defective in this direction. But we made the following experiments with the view of narrowing the question.

To test the experiments of Onimus, who maintained that the poison is not dialyzable, we constructed a dialysis apparatus with parchment. This apparatus, containing blood from animals dead and sick from inoculation with transmitted blood, was allowed to rest in water for twenty-four hours, and the experiments made which are described in the following Table:—

TABLE V.—*Blood of Dead or Sick Animals (transmitted blood) Dialyzed through Parchment, and after twenty-four hours the External Fluid Injected in the quantities given.*

No.	Number of Animals	Dose in Cubic Centimetres.	Died after Inoculation.		Survived and Employed for other Purposes.		Generation.
			No.		No.		
1	...	...	...	On 4th day,	...	...	8
2	4	0·8	4	" 5th "	...	...	10
3	...	...	...	" 4th "	...	...	10
4	...	...	...	" 2d "	...	...	12
5	2	0·4	2	" 15th "	...	...	9
6	...	...	...	" 2d "	...	...	8
7	2	0·16	2	" 3d "	...	...	8
8	...	...	...	" 3d "	...	...	8

N.B.—The controlling experiments made with healthy blood, similarly dialyzed, gave negative results.

We will not consider it as proved by these experiments that the poison itself is dialyzable. It is possible that putrefiable substances passed through the parchment, and afterwards putrefied in the external fluid. Our experiments, however, show that the inference that the poison is not dialyzable has no foundation.

We also boiled and filtered blood from the sick animals, and injected both the filtrate and the coagulum broken up in water. The results are given in the following Table:—

TABLE VI.—*Transmitted Blood Boiled and Filtered.*

No.	Number of Animals.	Dose in cub. cen.	Died after Inoculation.		Survived and Employed for other Purposes.		Generation.	
			No.		No.			
1	1	0·8	1	On 4th day.	...	...	8	The filtrate.
2	2	0·24	2	" 3d "	...	...	9	
3	...	...	...	" 4th "	...	...	9	
4	2	0·16	1	...	1	On 42d day.	9	Only boiled and not filtered.
5	...	...	...	" 4th "	...	...	11	
6	...	...	...	" 4th "	...	...	8	
7	...	...	...	" 4th "	...	...	8	The filtrate.
8	...	...	...	" 4th "	...	...	8	
9	6	00·8	5	" 2d "	1	...	8	
10	...	...	...	" 4th "	...	...	9	The coagulum.
11	...	...	...	...	...	...	9	
			9		2	" 20th "	9	

The surprising fact was ascertained by these experiments, that the poison is not destroyed by boiling.

The blood of affected animals was mixed with alcohol, and allowed to stand twenty-four hours, and the coagulum washed with water and injected. The three animals so treated all survived.

Further, we distilled the infected blood, and injected the product of distillation into two animals, both of which survived.

The most important result of our post-mortem examinations was, as has been already stated, the infiltration of the cellular tissue of the neck. Microscopic examination of the infiltrated fluid showed it to be full of pus-corpuscles, and an immense number of small granular elements, partly similar to those found, along with torn pieces of white blood-corpuscles, in our examinations of the affected blood.

In a number of cases, we found considerable swelling of Peyer's plaques, which, in one case, were ulcerated. In two cases we found pleuritic exudation. Pericarditis was observed several times. But none of these lesions were constant, the thoracic and abdominal organs being often found perfectly normal.

Our post-mortem examinations were, however, too defective to be entitled to much value in estimating the essential nature of the disease. Our chief attention was directed to the capital subject, whether the assertions as to the increased virulence of transmitted blood are to be accepted, and these assertions the experiments related above confirm.

The following Table shows the results of the few experiments we made on guineapigs:—

TABLE VII.—*Guineapigs Inoculated with Putrefied and Transmitted Blood.*

No.	Number of Animals.	Dose.	Died after Inoculation.	Survived.	Generation.	Inoculated from	
1	1	0.32	On 2d day.	...	1	Putrefied ox-blood.	No. 2 showed symptoms of severe illness the day after inoculation, and recovered in a few days.
2	1	0.32	...	1	1		
3	1	0.08	" 6th "	...	2	Blood from No. 1 ten hours after death. Blood taken from the ear of No. 2 the day following its inoculation.	
4	1	0.08	" 5th "	...	2		
5	1	Halfadrop	" 7th "	...	2		
6	1	"	" 9th "	...	2		

It will be observed from this Table that blood taken from a guineapig that was suffering from the injection of putrid blood, produced death in two others at the dose of half a drop diluted in water, and this although the guineapig that furnished the poisonous blood recovered. We met with an analogous fact in the rabbit.

The following experiments are too limited in number to afford a basis for any generalization, and I only give them here to be taken in connexion with those made in Paris with the blood of typhoid fever.

Blood was taken from the finger of a man in the General Hospital

at Vienna, who was in the twelfth day of typhoid fever, from which he died two days afterwards. It was diluted with distilled water, and injected into two rabbits. One received an approximative dose of 0·008 cub. centim., and died twenty-eight days afterwards. The other received an approximative dose of 0·08 cub. centim., and died twenty-two days afterwards. In both these rabbits putrefaction set in after death with unusual rapidity. To control the experiment, the blood of the rabbit which received the dose of 0·08 c. c., was injected into two others at the dose of 0·0008 c. c. One of these died eleven days afterwards.

Blood was taken from the finger of a girl ill of scarlet fever in the Children's Hospital in Vienna, and in the period of full eruption of the disease. The child died the following day. The blood was diluted with distilled water, and injected into two rabbits. One received an approximative dose of 0·0008 cub. cent., and thirty-eight days afterwards was set aside for other purposes. The other received an approximative dose of 0·008 c. c., and died twenty-one days afterwards.

Blood was taken from the finger of a woman ill of variola hæmorrhagica, in the eighth day of the disease, diluted with distilled water, and injected into two rabbits. One received an approximative dose of 0·00008 c. c., and died nine days afterwards. The other received an approximative dose of 0·008 c. c., and died fifteen days afterwards. From the latter, blood was taken and injected into another rabbit, in a dose of 0·0008. This rabbit died in two days.

ARTICLE VIII.—*Notes of a Case of Laceration of the Urethra with Hæmorrhage into the Bladder, from direct Injury.* By J. D. PRIDIE, Esq., M.R.C.S.E., etc.

(Read before the Medico-Chirurgical Society of Edinburgh, 15th January.)

ON the 24th July 1872, J. C., a well-grown young man, 18 years of age, while standing on a table cleaning a bird's cage, fell backwards some four or five feet astride a large washing-tub on the floor, the perineum alighting on one of the prominent handles. On recovering himself he found that he had passed a few drops of blood from the urethra. I saw him shortly afterwards; he then complained of severe pain in the course of the urethra, chiefly about the bulb. There was no mark or swelling at that time, where he had fallen. He had passed urine immediately before the accident. I saw him again in the evening, six hours afterwards. During that interval he had passed urine three times; the last two were each preceded by a few drops of blood, the urine then becoming clear. During the night he got out of bed; and, while standing on the floor, bleeding came on in a full stream from the urethra, which continued

till he fainted, and again returned on his recovering from syncope. I was sent for, and saw him at 2 A.M. On examining him over the abdomen, I found the bladder distended with a firm mass; the hæmorrhage from the urethra had stopped from sheer exhaustion. Up to this time hot moist flannels had been applied, which relieved the severe pain he suffered from at the outset. These were now changed for cold and ice, and a full dose of tincture of opium was given internally. Dr P. Heron Watson saw him with me in the forenoon, and cleared out the bladder of a large quantity of coagulated blood; the ice was continuously applied, and small doses of *ol. terebinthinæ* given every two hours. During the next two days there was no return of hæmorrhage except a drop or two of blood occasionally preceding the urine. On the third day the bladder became distended with clots of blood, with profuse bleeding from the urethra. Dr Watson again saw him, and kindly left with me an elastic bladder-syringe. After thoroughly washing out the coagulated blood from the bladder, a full-sized catheter was secured in the urethra. During the next ten days there were at least four returns of profuse hæmorrhage, the bleeding passing outside the catheter, but without any accumulation of blood in the bladder since the catheter was left in the urethra. The catheter was withdrawn, and re-introduced at intervals of three days, and ultimately discontinued fifteen days from the injury. The ice was continued over the perineum and bladder a few days longer.

In the case which is here narrated, the injury to the urethra appeared to be about the bulbous or membranous portions of the canal. A question arises—How did the blood from the laceration enter the bladder, as the arrangement of the vesical closure would rather determine the flow forwards than backwards? In elderly persons, bleeding may take place directly into the bladder from the distended prostatic veins. In the young subject, where these parts are comparatively healthy, as doubtless they were in this case previous to the injury, bleeding from these vessels is rare—although the previous history of this young man indicates an hæmorrhagic tendency—yet it is unlikely that the blood passed directly into the bladder from the prostatic system of vessels, as happens in elderly persons, while it seems more probable that the blood retrograded along the urethra into the bladder. This view is supported by the circumstance that, after the catheter was retained in the urethra, the blood ceased to accumulate in the bladder, although it was considerable along the side of the catheter.

The persistency in the return of bleeding, although characteristic of laceration of the walls of the urethra, appears also to have some connexion in this case with the constitution of the patient; if on any occasion he received a slight cut, the bleeding was stopped with difficulty and healed slowly. His mother, who died from phthisis, had a similar hæmorrhagic diathesis. The loss of blood during twelve days amounted to several pounds; and, constantly return-

ing, indicated a recourse to astringents internally—such as ol. terebinthinæ; liq. ergotæ; tinct. ferri perchlorid.; acid. sulphuric. dilut. These were taken at short intervals, generally every two hours. It is difficult to decide which, if either, of these had any effect in controlling the hæmorrhage. It was, however, while taking the acid. sulphuric. aromatic. that matters began to improve. His food at first consisted of milk and bread, with other farinaceous diet, and subsequently beef-tea and animal food; the bowels were regulated by enemata.

Part Second.

REVIEWS.

1. *Anatomical and Pathological Observations.* By JOHN GOODSIR. 8vo. Edinburgh: 1845.
2. *Cellular Pathology.* By RUDOLPH VIRCHOW. Translated by FRANK CHANCE. 8vo. London: 1860.
3. *The General Characters of Cells.* By S. STRICKER. In STRICKER'S "Human and Comparative Histology." 8vo. London: 1870.
4. *Case of Hypertrophy of the Spleen and Liver, in which Death took place from Suppuration of the Blood.* "Edinburgh Medical and Surgical Journal," October 1845. By JOHN HUGHES BENNETT. *Leucocythemia or White Cell Blood.* 8vo. Edinburgh: 1852. By the Same. *Text-Book of Physiology.* 8vo. Edinburgh: 1872. By the Same.
5. *On the Softening of Coagulated Fibrin.* By GEORGE GULLIVER. "Transactions of the Medical and Chirurgical Society," Vol. xxii. 8vo. London: 1839. *Notes on Morbid Parts.* "Edinburgh Medical and Surgical Journal," July 1843. By the Same.

IN the exercise of his duties, the critical reviewer has occasionally to take retrospective glances of science, in order to estimate the value of evidence in relation to the history of recent discoveries. Thus it was that the "Quarterly" reviewer of the year 1815 had to show that Prevost in Geneva had anticipated some of the results obtained by Dr Wells in England, concerning the nature and causes of dew; and similar examples might be indefinitely multiplied in physiological literature, as was shown in the Sydenham Society's edition of Hewson's works. Such ventilations, from time to time, are essential to the preservation of the purity of the we^{ll}

and stream of historical science, and desirable, if we intend to cherish the memory of the authors of the landing-places of scientific discovery, and—so to speak—to honour the creator in the creation. It is as duteous and generous for a nation to rejoice in its worthies as faithless and ignoble to neglect them; and the true patriot will never be indifferent to the fair fame of his countrymen.

And now, in this spirit, we mean to assert briefly the just claims of an eminent member of that band of illustrious anatomists by which our metropolis has been distinguished and adorned from the time of the Monros to the present day; and, in asserting the cause of justice in this case, to show that he has been subjected, and is still subjected, to most unworthy treatment by certain German physiologists, who have not scrupled to practise in like manner on some other of our countrymen. This task we undertake the more readily, because the wrong is by no means confined to the foreign and original perpetrators thereof, but is shared to a great extent by that large number of our native physiologists who have afforded—it is to be hoped ignorantly—either patronage or condonation to the offenders. The subject might appear somewhat stale, only, as already hinted, it has been freshened of late by a continuance of the offence; and this conduct, so far as we know, has not been met by any adequate check from the periodical press. If, in our late review (*Edinburgh Medical Journal*, April 1873) of the first volume of Stricker's Histology, the name of John Goodsir was omitted, it was simply because the conduct of the German writer appeared so vain and grievous as to demand emphatic notice in a separate Article.

In the summer of 1842 and winter of 1842-3, John Goodsir delivered, at our College of Surgeons, those memorable lectures, part of which, relating to the secreting structures, appeared in the *Transactions of the Royal Society* of our city, in 1842, and the substance of the whole in the work specified above. It was republished in 1868, in two octavo volumes, under the care of Professor Turner, and with a valuable memoir, by Dr Henry Lonsdale, of Goodsir's life, on which we have freely drawn for some of the facts in the present review; and have to express our gratitude to the biographer for his meritorious labours. In those works Goodsir announced important discoveries, which are well known, since they have become part and parcel of physiological science, and some of which we shall presently specify. They are all remarkable, not as examples of those fortuitous and happy events by which philosophers occasionally stumble on discovery, but as the successful result of careful and persistent research, legitimately disclosing truths which have much enhanced the fair fame of our city and the bounds of physiological knowledge.

In the year 1830 was published in folio, at Leipsic, one of the most important anatomical memoirs of this century, by Johannes Müller, and entitled *De Glandularum secermentium structura penitiori earumque prima formatione in homine atque animalibus*. All

the results that this illustrious physiologist obtained as regards the ultimate secreting structure were to the effect that this consists of the glandular follicles or tubules, thus mistaking the purse for the treasure within. He did not perceive anything further; and this was the view thereafter entertained throughout Germany, and, indeed, wherever physiological science was cultivated, until the advent of John Goodsir's researches in Edinburgh. There was, indeed, an observation by Bowman (*Lancet*, January 1842) of oily drops within the cells of a fatty liver; and a later description of cases of jaundice by Gulliver (*Edinburgh Medical and Surgical Journal*, July 1843, p. 54), in which the interior of the cells of the same organ were surcharged with biliary matter, chiefly collected around the nucleus of each cell. But these facts no way affected the originality of Goodsir's discovery, and its truthfulness was specially mentioned as supported by the presence of an excess of bile in those cells as a morbid condition.

Goodsir examined the nucleated cells of many glands of different animals, and found the characteristic secretions of these glands within those cells. Thus, he found milk in the mamma of the bitch; the inky matter in the cells of the ink-bag of *Loligo sagitta*; bile in the livers of *Helix aspersa*, *Uraster*, *Modiola*, *Pecten*, *Pirena*, *Phallusia*, *Aplysia*, *Buccinum*, *Patella*, *Nereis*, and *Carcinus*; uric acid in the kidney-cells of *Helix aspersa*; spermatozoa in the testes of *Squalus* and *Echiurus*; and the Tyrian dye in *Janthina fragilis*. Early in his inquiries, seeing the glandular secretion situated between the nucleus and the cell-wall, he supposed this last to be the secreting structure; but he soon corrected this error by assigning the secreting function to the nucleus. He observed milky chyle in cells; and this is a very interesting fact, as showing that the molecular base of the chyle is not a result of mere concoction or emulsion, but truly an organic secretion. By such observations, sagaciously designed and skilfully executed, the German doctrine, after an undisputed reign of many years, was finally consigned to the vast limbo of error; and the Scottish physiologist established the truth that "the ultimate secreting structure is the primitive cell." And this result of his researches he was ever enforcing; as he well might be justly proud of a discovery which marks an era in physiology, and discloses truth in one of her most recondite recesses.

But his observations on cells did not cease here. The latest conclusions as to the formation of cells by division, in opposition to the doctrine of their free development, are after his researches. He saw and clearly described the central cell, and all the other cells of its department deriving their origin from that central cell, this being the mother of all within its own territory; so that nutritive centres are merely cells which are germinal spots or departments, each containing simple or developed cells, all related to the central or capital cell, around which they are grouped; and thus this is the mother of all the cells within its territory.

Now the discoveries, so laboriously and successfully made by our countryman, have been made familiar in Britain through the Germans, who, so far from honouring the creator in his creation, have treated him with ignoble contempt. He deeply felt this indignity, and so did those friends who were most dear to him. And it is more in sorrow than in anger that we are obliged, by a continuance of the injustice to this eminent physiologist, to recur to the facts. When Virchow's work on Cellular Pathology was published, everybody admired the value of many of the views enunciated therein, but nobody seemed to care whence they came, except, indeed, Goodsir, and a few more physiologists, who stood aghast at the use of his discoveries and the suppression of his name. It was mentioned but once in the whole work, and then on a point quite beside the main subject; only, by a contrivance which looked very like what the French call *mauvaise plaisanterie*, the book was dedicated to Goodsir! How much he was affected by this mean treatment has been described by his worthy biographer, Henry Lonsdale. But we forget whether the sequel was related. However this may be, when the proposal was made to elect the German offender to the Honorary Fellowship of the Royal Society of Edinburgh, Goodsir's surviving brother, the Rev. Joseph Taylor Goodsir, published a very temperate and just remonstrance, now before us, and entitled, "Grounds of Objection to the Admission of Professor Virchow to the Honorary Fellowship of the Royal Society of Edinburgh" (2d edition, 4to, 18 pages, Neill and Co., Edinburgh, 24th December 1868). However, Virchow's influence prevailed over injured innocence, and he was elected to that Fellowship; and, so far as we remember, no proper notice was taken by the critical press of Virchow's conduct to Goodsir, save the honourable article thereon in the *British Medical Journal* of 12th January 1861.

How much he extended his researches into the domain of comparative anatomy and physiology is well known, and we have already intimated that as regards the ultimate secreting structure. Yet in the treatise on Cells, which is cited above from Stricker's work on Human and Comparative Histology, even the very name of Goodsir is, with a single and miserable exception, totally suppressed. But in spite of all such attempts to wipe the name of our illustrious citizen clean out from the records of that science which he has extended and adorned, Professors Virchow and Stricker may rest assured that the countrymen of Goodsir will not willingly let his name and achievements die. No doubt, we may be told that space was wanting for historical notices; but it was found for far less important names, and Stricker's book, if tried by its pretensions, will be found remarkably redundant and deficient. Space was provided in it for many errors, and for much extraneous matter which is not part of microscopic anatomy; and thus viewed, it is simply beneath criticism that in a work professing to be one of "Human and Comparative Histology" no room was found by the German for two or three lines as a mere act of justice to the worthy Scotch phy-

siologist. In March 1867, Goodsir was buried in the Dean Cemetery of Edinburgh, near the scene of those labours which he loved so well, and side by side with his friend Edward Forbes. Truly "these were honourable men in their generation;" like Saul and Jonathan, "they were lovely and pleasant in their lives, and in their death they were not divided."

In the late more lengthy than profitable discussions on tubercle, at the London Pathological Society, one of the speakers—we think Dr Charles J. B. Williams—truly remarked that some persons seemed to conclude that the giving of a new name to a part was tantamount to its original discovery. Thus, Hughes Bennett having used the old term suppuration in the title of his original case of an excessive and morbid accumulation of pale globules in the blood, connected with or related to an enlargement of the spleen, objection was taken that they were not pus-globules, and so we had afterwards such terms as leukhemia, leucocythemia, white cell blood,—as if mere verbal niceties could affect the date of the discovery, or anybody had ever shown a difference between pus-globules and pale blood-globules. These mere words are quite beside the question of originality, and, indeed, are little more than mist to hide the truth. And this, in the present case, is, that Bennett is the discoverer of what he recorded as, primarily, a suppuration of the blood, "independent of inflammation," in connexion with enlargement of the spleen; and that Virchow afterwards recognised the disease as a secondary one resulting from a peculiar affection of that organ. Thus far much credit is due to the German; only it is little in favour of his candour that he suppressed any reference to the old and excellent researches of Hewson, or acknowledgment of the undoubted merit of Bennett as the first discoverer of leukhemia. Indeed, a great value of the discovery is the illustration it affords to Hewson's views concerning the office of the spleen and lymphatic glands.

Again, Virchow gave the name of Thrombosis, thus pretending to its discovery, to the softening of clots of fibrin. But the discovery was an old one. It had been made as early as 1839 in England; and by a long series of experiments and observations, by which good proof was given that the softening of fibrin is an elementary and very important disease, quite distinct and of essentially different import from suppuration. But though all this had been well established many years before Virchow wrote, he has always, and very consistently with his other conduct, totally suppressed such truth, and conveyed the discovery, as if it were wholly his own, under a new name, to his book on Cellular Pathology.

With every respect for our German brethren, we cannot but feel, and think we have shown, that they have not been equally respectful of us; and we might complain more did we not feel that, however ungenerous they have been, our own younger physiologists have been even more to blame in ignoring the glorious achievements of their scientific ancestors.

A Treatise on Diseases of the Tongue. By W. FAIRLIE CLARKE, M.A., M.B. Oxon., F.R.C.S.; Assistant-Surgeon to Charing Cross Hospital. 8vo. Pp. 244. Illustrated. London: Henry Renshaw: 1873.

IN these days of special hospitals, where every organ has its own literature—almost its own reference library—it is not easy to find a new untrodden field. Mr Clarke has apparently done so, and though it is a little one, a sort of “Goodman’s croft,”¹ not very fertile to look at, he seems, in this book, to have cultivated it with very considerable assiduity, and to some good purpose. By the way, we may as well open the question as to what shall be the name of the special hospital for diseases of the tongue. Shall it be called after Læna—she never was canonized; or after Jeremy Taylor’s young friend, who bit out his tongue when terribly tempted? William the Silent, and John of the Golden Mouth, might each urge their claims. It is a difficult and perplexing question.

An introductory chapter on the Anatomy and Physiology of the Tongue, though clearly and pleasantly written, leaves us with the impression that the amount of our *exact* knowledge as to the source, distribution, and respective functions of the nerves of taste is still very small.

“That the chorda tympani takes part in conferring common sensation upon the tip of the tongue, is, as I have said, quite possible; and there can be no doubt that the integrity, not merely of the chorda, but of all the nerves that are in close association with the lingual of the fifth and the glossopharyngeal, is necessary for the perfect fulfilment of taste. When the *portio dura* is paralyzed, both the movements of the tongue and the sense of taste are impaired.”—P. 28.

On the examination of the tongue, Mr Clarke has not much that is new to state, except that he wisely insists on the value of ascertaining the reaction of the secretion, and the vegetation of the fur.

The chapter on Congenital Defects is very interesting, contains much amusing information, and some curious cases; a very careful account of the condition of tongue-tie, and the trivial operation required for its remedy.

Atrophy of the tongue, as a result either (1) of central paralysis, or (2) of paralysis of the hypoglossal nerve in its course, is then described. In the first set, the lesion is generally complicated with other nerve lesions, and often is bilateral. In the second, it is usually unilateral. Some very good cases are given, especially illustrating the second variety of paralysis, from Dupuytren, Paget, and Holthouse.

Hypertrophy is a much more simple, and, in a surgical point of view, interesting and important malady. Nearly always either congenital or occurring in the first few years of infancy, the treat-

¹ “Goodman’s croft,” a portion of land, generally small and useless, devoted to the author of evil, and left untilld.—P.D.

ment is complicated by the opposition of the patient, and is very difficult and disappointing. A careful table of forty-three cases, which had been treated in various ways, has been prepared by Mr Clarke. We find that compression, with astringent lotions and bandaging, succeeds admirably in cases where the patient is of an age which allows of his assisting the surgeon; but that, in the case of very young children, partial excision, ligature, or the galvanocautery, seem to be the best means of treatment. The author used the *ecraseur* in a successful case in a child aged five and a half months.

The chapter on Parasitic Diseases of the Tongue is satisfactory in its clearness of description. Besides the ordinary *Oidium albicans* and *Leptothrix buccalis*, a third very rare form, resembling the *Tricophyton tonsurans*, is described by M. Maurice Raynaud, and verified by the author.

Under the head of Inflammation of the Tongue many various conditions are grouped. Among others, the author verifies Mr Hilton's interesting observation, that unilateral furring of the tongue is generally associated with a morbid condition of the fifth nerve on the same side. The most marked case of unilateral furring the writer of this review has ever seen was in the progress of a very severe case of paralysis after diphtheria.

Glossitis proper varies according as it is the mucous coat only, or the muscular substance which is also involved. The latter is by far the most serious. It is well described, and some excellent cases given illustrating its origin in mercurial poisoning, exposure to cold, animal poisons, the exanthemata, and syphilis. As to treatment, the directions given are full, precise, and sensible.

Perhaps too much stress is laid on syphilis as it affects the tongue, for though such cases are very common—indeed, form nearly half of all affections of the tongue demanding treatment—still they are not separate diseases, but only symptoms of a constitutional poison, to the diagnosis and treatment of which attention must be directed. Still, those conditions are well described, and the plates illustrating them are well done.

A useful little bird's-eye view is given of the symptoms of Syphilis and Cancer of the Tongue respectively, which may aid a student in the diagnosis.

Cancer of Tongue has a chapter, and it is a sad one, to itself. Mr Clarke believes that "the minute investigations which have accompanied the recent progress of pathology, have proved that any other form of cancer, except the epithelial, is rare in the tongue; and that, even in those cases in which the disease is marked by an abundant admixture of fibrous tissue, the essential elements of the growth as revealed by the microscope are those of epithelioma."—P. 171. Notwithstanding this, Mr Clarke agrees with all surgical authority, that "Cancer of the tongue is a very deadly affection; and however completely the growth may be re-

moved, it is only too probable that the disease will ultimately return, either in that part of the organ which is left, or in the neighbouring glands."—P. 191. Early operation seems to be the key to any chance of success in treatment, and the chance is a small one.

The account of operations on the tongue is neither so full nor so exact as might have been expected.

A very interesting note on judicial excisions of the tongue in the East, and on the cases of martyrs who spoke after this mutilation, is given. It shows much care, erudition, and study.

Chapters on Tumours, Cysts, Salivary Calculi, and Accidents to the Tongue, conclude this very comprehensive and interesting work. An extraordinary accident is related by which a pipe-stem transfixed the tongue and penetrated the tonsil, transfixing the internal carotid artery and jugular vein. The patient lived four days, and died instantaneously of hæmorrhage when the pipe-stem was withdrawn.—Pp. 229-30.

We think Mr Clarke has produced a work of real value, and has entered on a field of special work which will bear fruit, so long as he remembers he is a surgeon, not a tongue-doctor.

An Introduction to the Study of Clinical Medicine: Being a Guide to the Investigation of Disease, for the Use of Students. By OCTAVIUS STURGES, M.D. Cantab. London: Smith, Elder, and Co.: 1873. Pp. 131.

THIS little book is simply an explanation of the *rationale* of taking medical cases. It is arranged according to a definite plan, but is likely to be extremely useful to all students who, in taking cases, must necessarily follow a similar, if not exactly the same, mode. Nor is it only to the student that this book is likely to be of use; many practitioners will find their diagnosis all the more clear, and their ideas of the nature of their patient's illness probably much more definite, from a little attention to the logical mode of inquiry laid down in it. There are one or two excellent hints which we do not recollect having ever seen in print before. One of these is, in auscultating the lungs, to ascertain, first of all, the individual standard of respiration, by commencing with the healthy lung—a most wise recommendation, which, in practice, is too often reversed. And the second is even, if possible, wiser; and that is, to educate the ear to appreciate the normal cardiac sounds and pauses, by beginning with the uniform tic-tac of the infant, and progressing gradually to the healthy adult. This mode of practical self-tuition is far before any other teaching. But this book is not perfect; there are one or two blots in it. We perfectly agree with the author that the terms, "note," "tone," "pitch," and "quality"

of sound applied to those sounds elicited on percussion, serve rather to confuse than to instruct; but we think it decidedly a long step backwards to return to Piony's absurd nomenclature—now discarded by himself—and speak of a pulmonary sound, a liver dulness, or a heart dulness. We may just as well speak of that proceeding from a bladder filled with urine as a humoral sound, though how that is to be distinguished from the sound elicited on percussing a bladder filled with a coagulum we do not know, a clot sound being, of course, the appropriate term for it. Nor is it one whit more correct to speak of a deep and a superficial dulness. The sounds produced by percussion depend entirely upon the physical condition of the parts percussed in relation to the amount of air contained in them. They are simply acoustic phenomena, and must be named accordingly; and it is not less confusing than unscientific to give these sounds names only to be understood by an adept in anatomy, a totally distinct branch of knowledge. Such a system of nomenclature implies the absurdity of giving a sound one name when it is produced on percussing the body, and another when we hear it elsewhere; for, of course, however much the sounds elicited might resemble each other, it would be preposterous to speak of a pulmonary or a jecoral sound as produced on the percussion of inorganic bodies. Nor is it less absurd to speak of a heart dulness in the region of the liver, and the reverse, as may sometimes be heard, or of one or other of these forms of dulness as to be heard where, in health, there ought to be a pulmonary sound. Apart from its unscientific character, it is folly to propound a nomenclature which may lead to such confusion. From the statements at page 84, it seems doubtful if Dr Sturges actually knows where the aortic and pulmonary valves really lie, and how their sounds are to be separated. It is certain that no definite statements are given in regard to either of these matters. He also states that, "we learn from books that the sounds of the dilated heart are 'weak,' those of hypertrophy 'loud and clear.'" Latham does not say so, and Latham is more accurate in this respect than many of his successors. On another page, Sturges says that "dilatation of the left ventricle carries with it, or makes highly probable, dilatation of the right ventricle also;" a statement which, if we exclude slight alterations, is only true in certain conditions and with certain limitations. He also states that a tricuspid bruit is often masked by the loudness of a coexisting mitral bruit; but a tricuspid bruit, though often absent when regurgitation is manifest, reveals itself when present always by some alteration in the tone of the bruit in tracing it from the mitral to the tricuspid area. Dr Sturges is a man who has clear ideas of the requirements of practical physic, one who cares much more for actual usefulness than for mere verbal refinement. We trust he will not regard our criticism as mere verbiage, for his book is too good to be spoiled, as it is, by the faults we have mentioned.

Lessons in Elementary Anatomy. By ST GEORGE MIVART, F.R.S., etc. London: Macmillan and Co.

THIS book, as was to be expected from an author so well known in the department of comparative anatomy as Mr Mivart, is an excellent one, and fully maintains the well-deserved reputation of its author. It is written in a thoroughly philosophical spirit, inasmuch as it not only embodies an immense amount of detail regarding the various modifications which the separate organs undergo in different animals; but the fact is never lost sight of, that underlying them all a certain unity of plan is distinctly recognisable. This renders the work more valuable, as it enables the reader to take a more comprehensive grasp than he would be able to do were he furnished only with isolated details, in the absence of a central idea round which they might be grouped.

The author takes as his standard of comparison the human organism as being best known, and, after describing it, proceeds to compare those of the lower animals, pointing out the various modifications to which they are subjected.

Such being the general plan of the work—and, as we have said, both in outline and detail it is excellent—we must express our opinion that as regards its fitness for the object which the author had in view in writing it—viz., to form a handbook “for teachers and earnest students of both sexes not already acquainted with human anatomy”—it must be pronounced a failure. The matter contained in this book is of such a nature, that we suspect that few who are even moderately well versed in human anatomy, will be able to master much of it without considerable application to the study of comparative and developmental anatomy; and if this is the case with such an one, what is to be expected of those who enter at once on the subject without any preliminary training whatsoever? We look upon the book much more as a “comparative anatomy,” and consequently adapted to those who have made a study of the subject, than as an elementary treatise suitable to those who know nothing of it, and much fear that before anatomy as a branch of ordinary education makes much progress, a very much more elementary work will have to be produced. The author is evidently accustomed to meet with those who are to some extent already conversant with the subject which he teaches, and forgets the preliminary difficulties which they had to encounter.

It is not easy to over-estimate the labour expended in condensing such an amount of detail as is contained in this work within so short a compass, more especially when the singular perspicuity of the style is borne in mind. It will, we are sure, be highly valued by all who are interested in the subject as a convenient book of reference; but as a text-book for general readers, we are afraid we must pronounce it unreadable.

The Comparative Anatomy of the Domesticated Animals. By A. CHAUVEAU, Professor at the Lyons Veterinary School. Second Edition. Translated and Edited by GEORGE FLEMING, F.R.G.S., M.A.I., etc. etc. London: J. and A. Churchill.

WE have often had occasion to lament the want of a thoroughly scientific and reliable English text-book of veterinary anatomy. Whilst in France and Germany such well-known works as those of Chauveau and Leyh have for many years been in the hands of students, in England, with the exception of that of Percival, which can no longer be considered adequate to the requirements of the present time, we have, until recently, had no work which could be regarded as a really good class-book. True, there have been two attempts of late to fill up the gap, the first of which must be considered a failure, inasmuch as the work has not been completed, and we fear that little more can be predicated of the second attempt, on account of the unsatisfactory nature of the compilation, although buttressed up by illustrations taken from standard works. We have little doubt that to this, to a large extent, may be attributed the very unsatisfactory position of veterinary anatomy in this country, as compared with that which it occupies on the Continent, and therefore feel that the author of the translation now before us merits the thanks of all who would wish this science to advance to that position which its importance in this, the leading agricultural country in Europe, demands. The value of the work is sufficiently well known, and this edition consists not merely of an accurate and, at the same time, free translation from the French original, but the author has, moreover, added to the descriptions where they seemed deficient, from other standard works in use on the Continent. It is therefore fully brought up to the present state of the science, and forms a text-book not only of the anatomy of the horse, but of most of the domesticated animals. The editor, with no small labour, has translated a book, the value of which is already recognised; the reward, we doubt not, he will ere long receive in seeing it adopted as *the* text-book in the various schools of anatomy, veterinary and comparative, in this country.

The Twenty-first Report on the District, Criminal, and Private Lunatic Asylums in Ireland, with Appendices. Dublin: 1872.

THE Lunacy Reports for the three kingdoms may be viewed in two aspects—as a collection of statistics bearing upon insanity, or as an annual review of limited circulation upon the management of the different establishments for the care of lunatics. In the Report before us there is a collection of useful statistics, apparently

judiciously framed to give information. The views stated by the Commissioners about the increase of lunacy are of interest. From 1846 to 1861 there has been an increase of nearly one-third in the number of persons known to be insane in Ireland, amounting in 1871 to 18,327, while the population has been diminished 2,772,365.

The extension of asylum accommodation has been going on steadily, without the number of lunatics at large ceasing to augment. Since 1869 there has been an increase of 1681 in the inmates of asylums and institutions for the insane, but the number of lunatics at large appears to have increased by 996. While making some allowance for the increased correctness of registration, it is difficult to resist the conclusion that the number of people who turn insane is somehow or other becoming greater every year. There is one question which is not mentioned by the Commissioners, but on which we should like to have had their opinion. To what extent the emigration of the able-bodied and well-to-do in Ireland may have had upon the residue of the population? We suppose they have left all their lunatics and most of their idiotic relations behind, and very likely some of those disposed to yield to the exciting causes of insanity. This must have something to do with the increase, though it is difficult to say in what degree.

We have looked over the table of dietaries of district asylums with much interest; the most noticeable feature is the absence or smallness of allowance of butchers' meat. In Armagh District Asylum, for example, there is no butchers' meat given, and the patients are noted as "remarkably healthy and well cared for." The average of death is 8.28 per cent. on the daily average number resident.

The returns of the names of governors of asylums and the meetings they attended, seem to us perfectly useless pieces of statistics, at least for publication, and some trouble must have been taken in compiling them.

The Commissioners give a very cautious opinion about the expediency of depriving dipsomaniacs of their liberty. "Some few of these cases benefit permanently from control and detention; others, and it is to be regretted, though full of promise when being restored to freedom, and philosophical even on the misfortunes attendant on excessive drinking, quickly lapse into their former habits. This class of patients, so difficult to be dealt with, and belonging mainly to the better grades in life, exists as it were on a neutral ground between sanity and insanity, holding a somewhat parallel position to that of convicts or individuals of a lower standing in the community, who, regardless of punishment, becoming reckless and uncontrollable in prison, are transferred to our Criminal Asylum as lunatics.

"To generalize on the treatment of either, or to fix, as the law at present stands, the proper confines of insanity, would be a task altogether beyond our power. Each case must be sepa-

rately considered, and as such exceptionally treated, even at the risk of *legal* proceedings for so doing, as, according to the statutes in force in Ireland, save in the instance of a person of unsound mind being confined in a private or public lunatic asylum by warrant of the Lord-Lieutenant, or by an order under signature of the Lord Chancellor, or unless the party can be distinctly sworn to as dangerous, the detention of any other lunatic is absolutely illegal, and an action lies against the detaining party, no matter who he may be—in other words, there are numerous actionable detentions this moment in Irish asylums, on which, according to a late decision in the Court of Queen's Bench, verdicts with damages might be given."

But if no general rules can be laid down, and each case must be separately considered, we cannot see how any general laws can be safely framed in the matter. The only resource is either to let things alone, or to erect a court with discretionary power to imprison dipsomaniacs, after examining their condition and hearing their cases.

The critical portion of the Report is much shorter than that of the Scotch Commissioners in Lunacy. It occupies 24 pages, whereas the Scotch one has 148 pages. This is accomplished by missing out the Commissioners' entries and other appendages. The Irish Commissioners, who perhaps have little time for essay-writing—there being only two for the whole of Ireland—content themselves with printing a short survey of the asylums, and the improvements which they have suggested, leaving their entries, which they apparently make on the spot, for the special consideration of the District Lunacy Boards.

An Introduction to Pathology and Morbid Anatomy. By T. HENRY GRACE, M.D. Lond., etc. Second Edition, revised and enlarged. London: Henry Renshaw: 1873. Pp. 363.

WE congratulate the author on the success of his book. This second edition has sixty more pages, and forty more woodcuts, than the last. These additions are not all improvements. There was not the slightest need for a chapter on the mounting of microscopic specimens, and the views on phthisis and its relation to tubercle are not made clearer than in the last edition, but that is perhaps scarcely to be wondered at. It still is the best little book of its kind in the English language; and, like all Renshaw's manuals, a perfect model in typography and getting-up.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LII.—MEETING VII.

Wednesday, 5th March 1873.—Dr HANDYSIDE, *President*, in the Chair.

I. The *President* showed the BRAIN OF A FEMALE, aged 34. The specimen was obtained from his dissecting-room. On opening the head, serous fluid to the amount of one half-ounce escaped from an old apoplectic cyst, with tense thick walls, situated in the right fissure of Sylvius. On the brain being removed, this cyst—evidently an old apoplectic one—still contained one and a half ounces of clear serous fluid, and more welled out from the anterior cornu of the lateral ventricle in communication with it. The situation of the cyst is on the under surface of the *right* frontal lobe, and it is adherent to Wagner's convolutions and to the island of Reil, both of which parts had disappeared. Farther, a hard, almost gritty, apoplectic cicatrice, a quarter of an inch long, of an elliptical form and a tawny hue, is situated above and behind the tail of the *left* corpus striatum. During the last eleven years, the patient had suffered from hemiplegia of the *right* side of the body, and but slightly of the left side of the face. She was morose and passionate, and did not articulate distinctly; but the aphasia was not well marked. She died of peritonitis four weeks after being delivered of a boy at the full time. The child was the subject of complete general paralysis, and died on the third day.

II. Mr G. R. Gilruth read a paper on FRACTURE OF THE SKULL, WITH PUNCTURE OF THE BRAIN, which appears at p. 36 of the present Number.

Dr Handyside thanked Mr Gilruth for his paper.

Dr Joseph Bell believed that many of the symptoms in cranial injuries were referable more to the contusion of the brain than to the injuries sustained by the cranial bones. He did not attach much diagnostic importance to the state of the pupil. He related the case of a child who had fallen from a height, in which he removed the greater part of the frontal bone on one side in consequence of fracture, and which ended in recovery. The worst cases of cranial injury were not necessarily those in which the bones were most injured.

Dr Chiene considered that the term concussion was most indefinite, and that many of the symptoms included under the term concussion were due to injury of the brain substance.

Dr Watson considered that the term concussion was only useful

as referring to a class of symptoms; as describing any pathological lesion it was most unsatisfactory. The state of the pupil was perhaps to be accounted for by some injury to the spinal cord in the upper dorsal and lower cervical region.

Mr Gilruth stated, in reply, that the brain in his case did not appear much injured.

III. *Dr Watson* read a paper ON A NEW OPERATION FOR ANCHYLOSIS OF THE ELBOW RESULTING FROM FRACTURE, which was published at p. 985 of the Number for May.

The *President*, *Dr Joseph Bell*, and *Dr John Duncan*, joined the discussion which followed. They considered that in certain cases *Dr Watson's* operation was a decided improvement, and congratulated *Dr Watson* on the success which had hitherto attended this modification. The difficulties of the operation were not great in *Dr Watson's* hands.

IV. *Dr Watson* showed, with the permission of the Society, a woman in whom he had excised a BRONCHOCELE of large size, by his new method of operation. Since the operation the woman's health had greatly improved. The scar resulting was of small size. On the right side of the neck, in the situation of the superior thyroid artery, there was a small pulsating tumour, probably indicating a communication between the artery and vein; it had gradually decreased in size since its appearance a week after the operation.

SESSION LII.—MEETING VIII.

Wednesday, 19th March 1873.—*Dr HANDYSIDE*, *President*, in the Chair.

I. *Mr Annandale* showed:—1. CARTILAGINOUS TUMOUR which he had removed, together with the upper third of the fibula.

2. PEDUNCULATED FATTY TUMOUR, with constriction at its centre.

3. DISEASED HEAD OF FEMUR, which he had excised in a case of hip-joint disease.

II. *Dr Argyll Robertson* then read a paper by *Dr Laidlaw Purves*, entitled SNELLEN'S OPERATION FOR STRABISMUS.

The conjunctival wound is made in the horizontal meridian, over the tendon to be divided. After separating the conjunctiva at the edges of the incision from the subjacent tissues, and loosening the caruncle from its attachments, the tendon of the rectus is raised with forceps and divided, along with Tenon's capsule.

The blunt hook is next passed along the surface of the sclerotic, so that any undivided fibres may be found and cut. The edges of the conjunctival wound are brought together by a suture. No chloroform is given except to children, and adults who insist upon it.

The advantages claimed for the operation are:—1st, The operation is easy of performance, and the danger of wounding the sclerotic is avoided. 2d, Tenon's capsule being cut equally, the

effect is in the proper direction. 3d, Pressure on the eyeball with the blunt hook is avoided and much pain prevented. 4th, The accumulation of blood under the conjunctiva is diminished; and, 5th, It is more in the operator's power to cut the capsule to the required degree, and so regulate the amount of the effect.

Dr Joseph Bell was of opinion that the danger of opening Tenon's capsule was more imaginary than real, as he had never seen any bad results follow the operation. In courageous patients, it was found unnecessary to give chloroform. He failed to see any particular advantage to be derived from the operation now recommended.

Dr Argyll Robertson remarked that one disadvantage he feared from the operation was, that a fungus protrusion might arise at the seat of the wound, in consequence of its being directly over the tendon. *Dr Snellen*, however, had only seen this happen in one case. On theoretical grounds he was of opinion that the sub-conjunctival operation, as performed in this country, is better than *Snellen's* operation. The separation of the caruncle appeared to *Dr Robertson* an advantage in *Snellen's* operation. He intended to give the operation a trial when a suitable case presented itself.

III. *Dr Eastwood* then read his paper, entitled DARWINISM IN ITS RELATION TO THE HIGHER FACULTIES OF MAN, which will appear in a future number of this Journal.

A discussion followed, in which the President, *Drs Craig, Chiene, Strachan, jun., of Dollar*, and the author, took part.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXII.—MEETING XI.

23d April 1873.—*Dr MATTHEWS DUNCAN*, *Vice-President*, in the Chair.

I. *Dr Keiller* exhibited TWO FULLY-DEVELOPED CHILDREN on which he had performed cephalotripsy. In the first case, the mother was only fifteen years of age, and was seized with violent convulsions at the commencement of labour. *Dr Keiller* tried to turn, but finding unusual difficulty desisted, and as another medical man had applied the forceps without producing any effect, he perforated the head and completed the delivery with the cephalotribe. The girl remained totally unconscious for twelve hours, but ultimately made a good recovery. In the second case, *Dr Keiller* found it unsafe to deliver the woman with the forceps. He turned, but was unable to extract the head till he had perforated it behind the right ear and applied the cephalotribe. This patient also made a good and rapid recovery.

II. NOTES ON DELIVERY IN CASES OF BREECH PRESENTATION, AND ON THE OCCURRENCE OF THE VAGITUS UTERINUS IN SUCH CASES. BY DR FROST. COMMUNICATED BY DR J. M. DUNCAN.

The following cases and remarks illustrate an improved method of delivering, through which the probabilities of saving the child are greatly increased by enabling it to breathe whilst its head is still in utero, and to utter cries proclaiming its living condition.

This mode of delivery I made known to my professional brethren a great many years ago, and taught in my lectures on obstetric surgery in the years 1851-52, in the Newcastle College of Medicine.

About thirty years ago, I attended a case in which the breech presented. The patient had previously borne children, and as the pelvis was well formed, and the soft parts dilated and dilatable, I expected to obtain a living child. The breech and shoulders passed in the natural direction—the finger was placed in the child's mouth and the funis drawn down; but at this critical period the pains ceased, and the child began to struggle—it was the death-struggle. Recollecting that, in the erect posture, the uterus acts more frequently and with greater power than in other postures, I requested the attendants to lift the patient out of bed, and set her upon the lap of a woman; this was not accomplished so soon as I could have wished, but when she was thus seated the pains at once returned, and after a short delay the head was extricated, though too late; for the child was dead, and all my efforts failed to restore animation.

Disappointed, I sat down to experiment upon a rude apparatus which I had had contrived for the purpose of instructing midwives; it consisted of a female pelvis covered with leather, padded, and with an elastic perineum, and a phantom fœtus, the head made of block tin, to which a lower jaw was attached by means of wire springs.

After many trials, I discovered that, whilst the child's mouth was opened by the finger of one hand, two fingers of the other, passed high up into the vagina, could, by exerting pressure in the direction of the coccyx, so open the passage as to admit into it air sufficient to enable the child to breathe, and thus establish its independent existence after the natural supply of blood was cut off by compression of the funis; and that this being effected, the accoucheur could safely await the uterine contractions, and thus avoid causing injurious strains upon the cervical vertebræ. Hence, I concluded that the following procedure in such cases should be followed.

The patient is to be placed on the lap of an assistant, the accoucheur in front, on a low stool or kneeling,—and if he were likely to remain long in that devout posture a pillow might be useful. In either of these positions he has the free use of his hands, and can extract in the direction of the axis of the pelvis, and especially in the axis of the outlet.

I had an early opportunity of testing the value of the above

mode of procedure. I attended a case of breech presentation; the previous labours of the patient had been protracted and laborious, and the children large. Having ascertained that the child was a male, and my patient having lost an only son, and being wishful to have a boy, I at once made her acquainted with the position of affairs, assuring her that she was in no danger, though there was danger to the child, and that if she would submit to be delivered out of bed the danger to the child would be greatly lessened. She readily consented. When the body and shoulders were delivered, and the child's mouth well opened by the finger of one hand—the body resting on my arm—two fingers of the other hand were introduced, and the vagina dilated so as to admit the air to the mouth. I felt the chest of the child begin to expand, and independent life was established. There was now no necessity to act precipitately or lay injurious strain upon the neck, and I could wait for the uterus to assist; there was heard a stifled cry, by no means so loud as I have since heard, and I think that the vagina had not been so fully dilated as on subsequent occasions I have caused it to be. The child was born alive, but the head had been so long detained, and the funis so long compressed, that I am of opinion that the child would have been lost if the mother had been delivered in bed in the usual manner.

To another case of lingering delivery through breech presentation I was called in by the late Mr B. Tulloch. I delivered the patient in the erect posture; there was some delay in extricating the head, but the vagina being well dilated, the child cried audibly whilst the head was in the pelvis.

In the case of a primipara in which I was consulted by the late Mr H. Harvey, the vagitus uterinus was similarly heard by him and by the attendants. In both these cases it was the opinion of my professional brethren that, owing to the delay in delivery and the compression of the cord, the children would have perished if the ordinary mode of delivering in bed had been practised.

But the most remarkable case of the kind was that of a phthisical patient of mine who had married and become pregnant. She presented unmistakable symptoms of lung disease, as irritating cough, purulent expectoration, dyspnoea, etc.

Her labour (the first) was tedious; unfortunately the breech presented, and as it remained high in the pelvis, with some difficulty a silk handkerchief was passed round the thigh of the child, and drawn through so as to make both ends equal. By means of this powerful tractor the breech was brought down to the perineum. As my patient was much exhausted with cough and urgent dyspnoea, and I feared hæmorrhage from the lungs, the delivery of the breech was effected as speedily as possible, and chiefly by my efforts. The finger of one hand being introduced into the child's mouth, and my assistant dilating the vagina with two fingers by forcing the parts towards the coccyx, a free passage was made for the ad-

mission of air; the child's chest began to heave, and respiration and independent life were established; the child soon began to cry, and was plainly heard by those present, who, believing that the child was actually born, were at hand with the needful appliances. The descent of the head was slow, owing to the feeble efforts of the uterus, and the cord had ceased to pulsate, but the child was born alive and lively.

Considering the long time the head had been engaged in the pelvis, we were led to conclude that the child would have been lost by the ordinary mode of delivery.

The mother perfectly recovered from her confinement, but died afterwards of phthisis.

More recently, in 1871, I was called by Mr Gilchrist to a case of breech presentation in labour, which had been going on for twenty-four hours.

The breech was arrested in the pelvis, the natural efforts insufficient, and the patient much exhausted. A handkerchief was passed around the child's thigh, and a good deal of force was required to extract the breech. My finger was then introduced into the child's mouth, and Mr Gilchrist with two fingers dilated the vagina so as to admit a rush of air; independent life was thus enabled to be established; the movements of the thorax of the child were felt, and crying commenced, causing surprise to my friend.

The delivery of the head was protracted and difficult, but aided by the uterine contraction. The child was born alive, and the imprint of the promontory of the sacrum was visible on the child's head, accounting for the delay in the delivery.

According to Mr Gilchrist's estimate, the head was from ten to twelve minutes detained in the pelvis. As the natural supply of blood was by compression of the funis cut off, I think I am warranted in saying that this child would have been lost but for the admission of air into the vagina during the passage of the head.

The system of delivery adopted successfully in the above briefly related cases, and taught publicly, has not been adopted by any one so far as I can learn. Many objections indeed have been made against it; thus some have been scandalized by,—

1stly, The indelicacy of the position which the mother is made to assume. Others have been alarmed,

2dly, At the possible rupture of the perineum; and others again,

3dly, At the prospect of hæmorrhage.

On each of these objections I will here offer a few remarks.

1. The first objection is a mere bugbear. I have seen as much or more exposure of the person in bed than in the erect posture. Some, for instance, have recommended slipping the back of the child against the abdomen of the mother; others, again, prefer going to the opposite side of the bed, in order that traction may be made in the direction of the outlet. Now, such means cannot be employed without complete exposure of the person of the mother.

Again, the usual position of the accoucheur is unfavourable to the performance of traction in the direction of the outlet—he is placed behind the patient, her right thigh is in his way; and should she draw herself somewhat into the bed, the embarrassment is increased.

After a study of all the circumstances, it has occurred to me that an arm-chair might be constructed with arms removable at pleasure, and back so arranged as to allow of the patient being fixed at any angle or in the horizontal posture, and with the seat open in the middle, but with margin just enough to support the nates.

Upon such a chair by the bedside, the patient, with suitable drawers on, could be easily placed and brought into the most appropriate and favourable position for instrumental delivery or version; and after delivery could be as easily lifted back into a clean dry bed. In this way exposure would be reduced to a minimum.

In some difficult forceps cases, I have placed the patient on her back, the breech resting on the foot of the bed, the legs being supported, and the head and shoulders elevated with pillows, and thus not more than an absolutely necessary exposure has been incurred.

2. Laceration of the perineum. This sad accident has happened to me once in the course of a long practice, and it occurred during delivery in bed. The case was one of placenta prævia, in which, the placenta completely covering the os, the hæmorrhage was profuse.

I had not then been many years in practice, and the gentleman, whose pupil I had been, was consulted in the case; he confirmed the diagnosis at which I had arrived, and agreed with me on the necessity of immediate delivery; but on my requesting him to operate, replied, "You must do it yourself!" I passed in my hand by the side of the placenta, and not through it as some have absurdly advised, and alarmed by the hæmorrhage, whilst anxious to save the child, I effected its delivery with all possible speed. One finger was placed in the child's mouth, and the other over its shoulder, a pain came on, and the head was expelled with a jerk, lacerating the perineum through the sphincters into the rectum. The child was born alive, and lived to become a member of our profession.

3. Hæmorrhage. I have seen enough of this during delivery in bed, but have never seen it so profuse as to create alarm when the delivery has been conducted out of bed, and as I have herein recommended.

On my first adopting this plan, I used, fearing hæmorrhage, to put the patient to bed as soon as the child was born, but latterly I have not so removed her until the placenta is expelled, for by compression of the uterus with both hands the placenta is soon forced into the vagina. So satisfied, indeed, am I of the efficacy of such pressure in the erect posture, that, in cases of inertia uteri (in which

no morbid adhesion exists), I believe that the necessity for the introduction of the hand may be obviated.

Many years ago, before the use of the syringe for injecting cold water into the uterus, I attended a case of dangerous post-partum hæmorrhage—the placenta extracted, the uterus emptied of coagula, the cold douche used liberally, the bleeding persistent, my patient's life in imminent peril—I filled a spouted jug with cold water, dilating the vagina with my left hand, poured the water in a full stream and with a jerk into the vagina, hoping the water would enter the uterus, as the patient's head and shoulders were laid low; this at once arrested the hæmorrhage. The syringe is not always at hand, the jug always; moreover, I think the jug is to be preferred, as a larger stream can be thrown in at once, and continued if necessary. Although my patient lay for many hours in a bed soaked with water, she recovered well, save debility from loss of blood.

Dilatation of vagina for the admission of air. The bent speculum for examining the fauces might be tried; the blade occupies little space; the handle held by an assistant (nurse), the hand would be out of the way, withdrawing the blade as the head descended. Hitherto the fingers have answered, but they are bulky.

Dr Duncan spoke of the advantage of admitting air into the lungs before the head was born, and thought it a practice too much neglected. He could not see the advantage of having your patient out of bed, as he believed air could be admitted more easily in other positions. *Dr Frost* attached too much importance to the compression of the cord; it is not the compression which causes the asphyxia, but the empty state of the placenta.

III. ON THE CHANGES UNDERGONE BY THE CERVIX UTERI DURING LABOUR. BY DR MATTHEWS DUNCAN.

This paper appeared in the June No. of this Journal, p. 1066.

Dr Macdonald believed in the accuracy of the anatomical relations, and that the cervix persisted up till the last fortnight, perhaps only up to the last month, in primiparæ.

Dr Duncan thought the great question now was, How far can the placenta be separated before the child's head is pushed through the os, in cases of placenta prævia?

SESSION XXXII.—MEETING XII.

Wednesday, 14th May 1873.—*Dr Simpson*, Vice-President, in the Chair.

I. *Dr Simpson* exhibited THREE FIBROUS POLYPI which he had lately removed from patients who had been suffering from considerable hæmorrhage for some time.

II. Discussion on METRORRHAGIA.

SESSION XXXII.—MEETING XIII.

28th May 1873.—Dr RITCHIE in the Chair.

Dr Cairns read a paper entitled A PECULIAR CASE OF ABORTION.

A woman who had borne five children, and previously had never had an abortion, sent for me one night about 12½ P.M. Labour pains were evident. The mammæ were quite flaccid however, and no *areola* round the mammillæ; "she had altered" about six weeks previously. The urine, so far as I could judge, was quite normal. She had had no morning sickness, and felt no desire for any particular food, but simply complained of pain in the "belly."

When I called on the following morning, I considered it to be my duty to make a vaginal examination. I did so, and found something protruding from the "os."

I immediately applied the dressing forceps, and removed what, on after examination, turned out to be a fœtus of about six weeks old.

Eight months after, I was called to the same patient, and delivered her of a fine healthy child, which I am prepared to show to the Society if they wish, along with the *aborted case*, which I have in a bottle.

Having read his paper, *Dr Cairns* pointed out that the case was of interest for two reasons. First, that it showed how one ovum might be thrown off at an early stage of pregnancy, while the other was retained till the full time. And, secondly, in a medico-legal point of view, it proved the difficulty of determining whether a child was legitimate or not, the husband being dead, and the wife having a live full-time child, after she had aborted.

Part Fourth.

PERISCOPE.

ON CHRONIC POISONING WITH CHLORAL HYDRATE. By DR LUDWIG KIRN. (*From the Allgem. Zeitsch. für Psychiatric.*)—The immense literature of chloral hydrate, which represents, however, but a fraction of the cases actually observed, affords many references to acute poisoning involving the greatest danger to life; or ending fatally in spite of remedies. Cases of the latter kind have appeared, especially in the English journals, with a truly fearful frequency. . . .

The most prominent of the morbid symptoms were those connected with the skin; their production for the most part depended on the cutaneous vaso-motor nerves. They were more or less extensive erythemas, and pustular or papular exanthemata.

The clinical observations of Schüle on chloral erythema must here take the first place. "The use of chloral produces a tendency to fluxionary hyperæmias, with increased and strengthened heart-action. This is first and most considerably manifested in the head, the vessels being dilated, and an intense erythema occurring, at first in spots, but afterwards more diffusely. In the more pronounced forms the erythema extends downwards to the trunk, and becomes general, in which case it seems to follow, by preference, the course of the larger nerve-trunks. This chloral-rash remains latent until it is set going by some stimulus to the vascular system, but then appears in an intensity and rapidity which are proportioned to the existing current of (general) chloralization."

I reproduce these results of Schüle's verbally, because I can entirely confirm them. Although, from the varying chloral idiosyncrasies of individuals, they are not always observed, the statement is true for a great number of cases, and I have at this moment a series of patients under my eyes in whom the chloral-rash can be produced with the certainty of an experiment. . . .

The effect is not always limited to congestion and erythema of the skin, but other skin affections are occasionally produced; for instance, our female patients, who had taken 60 grains of chloral daily, first had erythema of the face, and later a papular rash on the arms, with red bases. In some nettlerash occurred. . .

With these affections of the skin must be associated affections of the mucous membranes, which, so far as they are not the result of direct chemical irritation, must be considered analogous to the former—i. e., they are produced by paralysis of the vaso-motor centre. For instance, in some patients treated with chloral we observed redness, swelling, and secretion of the conjunctiva, which became severe if the medicine was continued, but speedily disappeared on its disuse. Other observers, especially Balfour and Brady, have noticed this conjunctivitis. In three of our patients, probably from the mechanical irritation of the medicine, there was moderate catarrhal sore-throat, with pain in the fauces. In one of Curschman's patients there was pronounced redness and swelling of the epiglottis and of the false vocal cords. . . .

An important symptom which we have noted in a series of cases of the long-continued use of chloral is an interference with respiration, which may remain slight and scarcely troublesome to the patient, or may become positive dyspnoea. This symptom was experimentally produced by the Swede Hammersten, who observed severe dyspnoea in cats that had taken chloral, and was briefly noticed by Jastrowitz, one of whose patients, while taking chloral, suffered from severe dyspnoea with occasional cessation of breathing; and it was finally completely described and explained by Schüle, who observed a patient who,

after long use of chloral, used regularly to suffer after meals from a sense of oppression which made going up stairs extremely difficult, and even interfered with speech, although there was no chest disease to account for this. The symptoms persistently recurred in spite of all treatment till the chloral was left off, when the oppression entirely disappeared.

A similar chloral dyspnoea, though not so long continued, occurred in many cases observed by us, either with or without a rash, and a feeling of heaviness and anxiety.

That the chloral dyspnoea does not always stop at the lower degrees, but may proceed to the most severe and dangerous developments, is shown by the following observation communicated to me by an eminent physician. This gentleman was summoned in consultation to a lady prostrated by long sufferings, who had of late suffered from attacks of extreme dyspnoea, which had increased to asphyxia; at the same time the face was swollen, the facial muscles paralyzed, and there were also all the signs of cerebral effusion. Every remedy had failed, and the patient seemed on the brink of the grave. The physician therefore recommended the discontinuance of a daily dose of 45 grains of chloral which had been given as a hypnotic; whereupon all these highly alarming symptoms vanished in an almost magical way, the cerebral disturbance ceased, and the respiration quickly resumed its normal type! . . .

We come now to a fourth group of cases in which both the quality of the symptoms and their greater or less extension in the organism indicate a distinct change in the composition of the blood. In this connexion the following cases observed by Crichton Browne may first be referred to; their interesting phenomena justify a detailed report:—

CASE 1. A woman, aged 69, suffering from periodical mania, had 20 grains of chloral thrice daily: on the fourth day a redness was developed on the skin of the chest and shoulders, which did not vanish on pressure; on the sixth day the eruption had extended over the whole trunk and limbs, livid spots and deep red patches alternating. The lips and the mucous membrane of the mouth were excoriated, the gums spongy, the tongue blistered and ulcerated, the breath foetid. The general state was one of great depression; pulse 120. On the eleventh day the ulceration of the mouth had extended further; the lips were covered with crusts. The petechial eruption was diminished on the chest and abdomen, the spots were yellowish with patches of white skin between them; the spots on the arm lost their redness later. On the fifteenth day there was a sort of general desquamation; fissures of the skin over the sacrum, and in the neighbourhood of the joints. From that time convalescence proceeded, and ordinary health was restored.

CASE 2. A woman, aged 46, suffering from cardiac disease,

hemiplegia, and dementia, took 15 grains of chloral three times a day, with a calming effect. On the nineteenth day of the treatment numerous purple-red spots appeared in the neighbourhood of the left elbow; on the next day many similar spots were seen on the shoulders and fore-arms, which coalesced with the others. On the twenty-first day livid spots came on the face; the left arm swelled and became hard; on its reddened surface appeared a multitude of minute points of a much deeper colour, which did not diminish on pressure. Next day there were dark purple spots and discolorations, some small, round, and circumscribed, others broad and irregularly shaped, on the legs and abdomen, and in stripes on either side of the vertebral column. Simultaneously with the petechiæ there were great prostration, tendency to somnolence, weakness and excitability of the pulse, sore lips, thickly coated tongue. On the twenty-third day the spots and discoloured patches had extended in every direction, and the previously bright red spots had assumed a deep purple colour. Finally, signs of lung-congestion appeared, with gradual failure of power, and death, after several fainting fits, on the twenty-sixth day. At the autopsy numerous ecchymoses of every shape and size were observed more or less on all parts of the skin; the right lung was congested and oedematous, the heart dilated and its valves thickened; over the right central hemisphere there was a large arachnoid cyst containing fluid blood.

With the foregoing may be joined a case related by Monkton, in which, after four days' administration of 60 grains of chloral daily, a rash resembling slight variola with hæmorrhagic purpura appeared, and death occurred on the sixth day by syncope. Finally may be mentioned two patients of Pelman, in whom, after treatment with chloral, there were larger and smaller petechiæ over the whole skin: in one, which proved fatal, numerous petechiæ were found on the mucous membrane of the larynx and under the endocardium; and a hæmatoma on the right side in the skull reaching to the base, the fluid contents of which gave evidence of their recent origin.—*Practitioner*, June.

POISONOUS DOSES OF CHLORAL.—The smallest dose which has produced poisoning is $2\frac{1}{4}$ scruples, while 460 grains have been taken without unpleasant consequences. The symptoms are diminished frequency of respiration, redness of the conjunctiva, contraction of the pupils, lividity of the lips, and falling of the jaw. The character of the pulse has not been found to be uniform. The treatment should consist in—1. Removing the poisonous matter, or diluting it by water, tea, coffee, or other fluids; and, 2. Restoring respiration. Too much reliance must not be put upon so-called antidotes, such as strychnine, physostigmine, morphine, camphorated ether, and ammonia. These remedies cannot always be trusted. Possibly transfusion of blood, which has been successful

in cases of poisoning by chloroform, may also prove useful in these cases.—*Med. Chir. Rundschau*, Oct. 1871.

PODOLINSKI ON THE EXPULSION OF CARBONIC OXIDE AND NITRIC OXIDE FROM THE BLOOD.—In opposition to commonly received opinions, Donders recently stated that carbonic oxide could be expelled from its hæmoglobin compound by other gases; and Zuntz has separated it by means of the vacuum. S. Podolinski (*Pflüger's Archiv*, vol. vi., part 11), under the direction of Hermann, compares the comparative displacing power of hydrogen and oxygen on the carbonic oxide compound, and also on the nitric oxide compound, with hæmoglobin. He finds, on operating on small quantities of carbonic oxide blood with hydrogen, in a manner which allows complete mixture of the two, that half an hour is sufficient to expel all the carbonic oxide from blood saturated with this gas. Atmospheric air acts still more rapidly. In regard to nitric oxide in blood, hydrogen alone was used to expel the gas, in order to avoid formation of nitric peroxide. In this case, also, the compound was completely broken up and nitric oxide expelled, but longer time was necessary. Hence, Donders's discovery that carbonic oxide can be expelled by indifferent gases is extended to nitric oxide; and therefore the relation of hæmoglobin to the gases combining with it is, that oxygen, carbonic oxide, and nitric oxide can be expelled by indifferent gases, the degree of difficulty increasing in the same order. Each of these gases can be expelled by the one immediately following it with more ease than by an indifferent gas, and apparently, also, each is more easily evolved by the action of the one immediately preceding.—*Medical Times*.

DIGITALIS AN ANAPHRODISIAC.—M. Gourvat, in the course of a paper published in the *Gazette Médicale de Paris*, on the action of digitalis, says, "When digitalis or digitalin is administered for some time to a man in full possession of sexual powers, these become gradually weakened, the propensities disappear, formation of the liquor semenis diminishes, and may at last cease altogether. The anaphrodisiac properties of the drug are the secret of its good effect in spermatorrhœa."

EMETICS AND NAUSEANTS.—*Le Mouvement Médical* quotes from the *Bulletin de Thérapeutique* of 15th March the following remarks, from an article, by M. D'Ornellas, on the physiology of vomiting: "The distinction between emetics pure and nauseant emetics is not altogether natural, since characteristic examples of each variety are rare, and the two effects are generally found combined in the same substance. M. Gubler thinks that an emetic is energetic in proportion to its nauseousness, acting not only on the stomach, but also on the buccal, nasal, and pharyngeal cavities. There is no doubt that the direct action of some articles is due to a different substance from that which produces the indirect. Thus the nauseant action

of ipecacuanha is due to a principle having odour, and separable by ether (Magendie and Pelletier), the emetic influence to emetia, separable by water, and especially by alcohol (Magendie). These substances act as follows: The emetia acts always directly upon the gastric mucous membrane, upon its sensitive nerves, while, on the other hand, the nauseant principle acts upon the nerves of special sense, of the tongue, pharynx, and nose, and causes vomiting as soon as it is swallowed, or even before, independently of any local gastric action, or of elimination."

SIRÉDEY AND CORDIER ON THE TREATMENT OF ANASARCA, ASCITES, AND OBSTINATE PLEURITIC EFFUSIONS BY MILK.—MM. Sirédey and Cordier (*Journal de Médecine et de Chirurgie Pratiques*) have used this old and rather favourite remedy against dropsies in a way which they consider renders it less troublesome and repugnant than the "milk cure" is otherwise apt to be. At the same time that Sirédey prescribes two pints of milk a day to his patients, he allows other kinds of food and drink. Cordier, in his thesis, cites cases of albuminuric dropsy in which the diuresis was rapid and considerable, and the dropsy disappeared under this treatment. The diet is otherwise moderate, including meat and a little wine. He does not give more than two or two and a half pints of milk, at convenient intervals, in the day. He prefers asses' milk.

ISNARD ON THE TREATMENT OF CONSTIPATION BY ARSENIC.—Dr Isnard (*Marseille Médical*, 1872) states that many other physicians besides himself have observed that arsenic excites the appetite and improves the digestion, and facilitates the action of the bowels, but he is not aware that it has been methodically applied for the relief of constipation. Besides (1) improving the appetite, it (2) excites the muscular action of the intestines, and (3) it augments the secretions of the alimentary mucous membrane. It is particularly useful in the constipation of debilitated and anæmic females, sedentary persons, and old people. He administers arsenious acid in the dose of six to ten milligrammes, twice or thrice a day, with food. Sometimes smaller doses suffice. It may be used steadily for some time.

FUMING NITRIC ACID FOR INTERNAL PILES.—Billroth records 26 cases of prolapsing piles treated by him in various ways. In four instances he applied the actual cautery, in ten the galvanocautery, and in the remainder fuming nitric acid. The latter plan was pursued as recommended by Dr Houston of Dublin. The results proved eminently satisfactory. His mode of proceeding was as follows:—A free evacuation of the bowels was obtained by means of castor-oil given the day previously. Before the operation, the mass was brought down by an injection. The patient was then placed on the side, with the knees flexed. The parts adjacent to the anus were first well protected by oil, so that no injury

should be done them. A small piece of wood was then dipped in the acid and applied to the outside of the swollen mass, until it had become tolerably stiff, and had assumed a yellowish-green colour. It was then smeared with some simple form of ointment, and returned within the sphincter. The operation was usually performed without an anæsthetic, and an opiate suppository was rarely given afterwards. It is proper to keep the patient in bed. Fever rarely follows, though retention of urine is not uncommon for the first few days. The eschar usually separates without loss of blood. It is proper to give castor-oil on the third or fourth day, provided no fæces have passed. Hæmorrhage will be likely to occur if the fæces become hardened: such accidents, however, are readily controlled by ice. Of the patients treated in this way, some were discharged on the fifth and ninth days, though severe cases were under treatment from six to eight weeks. Several of the patients were examined a year after the operation, and there was no stricture in any one of them. Billroth believes that in very severe cases this treatment may fail, and then suggests the use of the acid nitrate of mercury, as recommended by Curling.—*Wiener Med. Wochenschrift*, No. 35, 1871.

9. EMBOLISM IN ENDOCARDITIS.—The following statistics have been compiled from the records of the Pathological Institute in Berlin. They are the results of autopsical examinations in 300 cases of endocarditis that were observed between the years 1868 and 1871. The lesions were confined to the valves in nearly every instance; disease of the parietal endocardium occurred only once. The affection was limited to the right side in 1 per cent. of the cases; in 9 per cent. it was associated with similar trouble on the left side. In 10 per cent. the left valves were the only ones diseased, but in 89 per cent. the right valves were also affected. The following facts were also ascertained:—The tricuspid was diseased alone in 1 per cent. of the cases, and with others in 10 per cent. The pulmonary was not diseased alone, but in 1 per cent. with other valves. The mitral was alone affected in 52 per cent., with others in 85 per cent.; the aorta alone in 13 per cent., with others in 43 per cent.: 29 per cent. of all the cases were complicated with emboli. In 2 or 3 per cent. of these the trouble originated in the right side of the heart, and produced infarctions and abscesses in the lungs, chiefly in the lower lobes. In 26 per cent. the emboli were derived from the aortic circulation. In the 76 cases of this nature, the mitral was affected in 87 per cent., and the aorta in 49 per cent. The following is the ratio of relative frequency with which emboli occur in three different organs:—In the kidneys, in 75 per cent.; in the spleen, in 51 per cent.; in the brain, in 20 per cent.; in the intestinal tract and in the liver, in 7 per cent.; in the skin, in 5 per cent.; in the medulla, in 3 per cent.; and less often in the thyroid gland and eye.—*Med. Chir. Rundschau*.

IN-FLESHED TOE-NAIL.—Dr B. E. Cotting, Harvard (*Boston Medical and Surgical Journal*, January 2, 1873), gives a new operation for this painful and persistent affection, with illustrations, which consists in removing all the diseased parts, together with quite a large piece of the sound flesh, skin deep, from the side of the toe—sometimes making an open wound, say nearly an inch long by half or three-fourths of an inch wide. No portion of the nail need be removed; but if, in order to fully secure all the diseased flesh, overlapping or undergrowing, a segment of the nail is involved in the cut, no harm comes of it. The result, he states, is quite as good, perhaps better. By this operation, all the diseased parts are removed at once, and a clean, healthy wound substituted; to be treated as any other open freshly-cut wound, and to be allowed to heal as soon as possible by granulation. Generally, the healing is rapid, and without interruption. In the second place—and this is the principle on which success depends—as the comparatively large and superficial wound heals, there is a contraction of the parts and a drawing-in of the skin towards the centre from all sides, including that near the nail; so that when the wound is healed there is nothing left in the way for the nail to impinge upon its subsequent normal growth. The shape, also, of the toe itself is usually much improved by the operation.

DIARRHŒA IN TEETHING.—In a clinical lecture “On the Primary Dentition of Children,” by Francis Minot, M.D., Harvard (*Boston Medical and Surgical Journal*, January 2, 1873), in speaking of the diarrhœa complicating teething during hot weather, recommends the common chalk-mixture, with the addition of one-fourth part of tincture of kino, which increases its astringency, and also keeps it from turning sour in hot weather. If the diarrhœa be not checked by this mixture, one drop of laudanum may be added to a dose, but not oftener than three times a day, in children under two years old. Diarrhœa is most apt to attack children who are brought up on the bottle; hence, if the case be urgent, and do not yield to treatment, a wet-nurse should be procured if possible. When this cannot be done, he would strongly recommend the method of preparing the milk with arrow-root and gelatine, found in the treatise on “Diseases of Children,” by Drs Meigs and Pepper. Brandy is very useful to a teething child exhausted by diarrhœa, which should be given once in three or four hours, or oftener in urgent cases. The dose is ordinarily from five to twenty-five drops, given in milk; but if there be much prostration, the physician need not fear to increase the amount.

ACTION OF COLD WATER ON THE SPLEEN.—The importance which many authors, and Fleury especially, have ascribed to cold water as a remedial agent in intermittent fever and various other affections of the spleen, has induced Prof. Mosler, of Greifswalde, to

test its efficacy, and he has arrived at the following conclusions:—

1. The direct contact of cold water with the normal spleen causes it to contract appreciably, and the degree of contraction varies according to the temperature of the water and the length of time for which it is employed.
2. Cold water also acts similarly on the normal spleen, if applied over the abdominal walls, but it is less effectual. In such cases the cold douche is to be preferred to cold compresses or bits of ice, but in all of them it accomplishes less than quinine.
3. In all forms of enlarged spleens, the result of disease, whether acute or chronic, the action of cold water causes contraction of the organ.
4. The paroxysms of intermittent fever can be arrested by Fleury's method of using the cold douche, but it has no advantage over quinine, either in new or old cases.
5. The cold douche does not accomplish a permanent cure of this fever. It does not always prevent recurrence, and leaves the spleen enlarged.
6. The spleen of typhoid fever is diminished in size by the same remedy.
7. In all forms of enlargement of the spleen, whether acute or chronic, the application of cold water to the spleen, either in the form of cold baths, ice-bags, or cold douches, with the internal use of quinine at the same time, is to be preferred to either of these methods alone.
8. As a substitute for quinine in chronic cases, quinoidine is recommended as being very much cheaper.—*Archiv. f. Ath. Anat.*, lvii. 1.

THE PULVIS GLYCYRRHIZÆ COMPOSITUS.—The compound liquorice powder is a preparation of the Prussian Pharmacopœia, especially useful in simple constipation resulting from atony of the bowel. It is composed of the following constituents, so prepared as to form when incorporated an almost impalpable powder: Senna leaves, ℥vj.; liquorice root, ℥vj.; fennel seeds, ℥iij.; sulphur, ℥iij.; refined sugar, ℥xviij. The usual dose is a small teaspoonful at bedtime, in water, forming an agreeable draught.—*New York Med. Rec.*

NEPHROTOMY.—W. W. Dawson, M.D., of Cincinnati, Ohio (*N. Y. Med. Jour.*), reports a case of extraction of a calculus from the kidney of a married woman, aged 50 years, who had borne nine children, and had always been a healthy and robust woman until eight years ago, when she had an attack of hæmaturia. The patient died on fifth day. The calculus was quite light, weighing only 20½ grains, and measuring seven-eighths of an inch in the length by half an inch in breadth at its widest part, being uniform in shape. It was ammoniac magnesians phosphate, and situated on the internal wall at a point corresponding to the hilus of the kidney, and was embedded in partly disorganized fibrin. The kidney tissue which he incised was about one half-inch in thickness, and bled but little.

THE EARLY SYMPTOMS AND TREATMENT OF INSANITY.—In a paper read before the Bristol North District Medical Society, Dr W. W. Godding, Physician to Lunatic Asylum, Taunton, Mass.

(*Boston Med. and Surg. Jour.*), gives the following symptoms and management of insanity:—In nine cases out of ten, there will be marks of disorder about the clothing or person in some way, for, as a rule, the insane show untidiness of dress; the insane are often uneasy, in a constant motion, getting up, walking about the room, busy with nothing. Later in the disease, they will sit for hours in the same position; sometimes this is so from the commencement, not usually. Depression, as shown in the whole manner as well as in the countenance, is seen in the early stage of insanity, in a majority of cases. Dr Saukey goes so far as to claim that no attack of insanity occurs primarily without an incubating stage of depression. He doubts the truth of this, but it is a very common condition. Hallucinations of hearing are not very uncommon in insanity, and must be regarded as an unfavourable symptom. Hallucinations of sight are, in his experience, much rarer in insanity than in delirium. Want of sleep occurs in a great majority of the cases of insanity, and it is surprising how long they will sometimes survive apparently without sleep, and yet in the end recover. The return of sleep is often the commencement of convalescence. The digestion is impaired in the early stages. Sometimes the moral sentiment alone seems to be at fault. A not very uncommon form of this is seen in young persons recovering from typhoid fever. Paresis seems the very inflorescence of insanity. In regard to the early treatment, in the active forms of insanity, the two important points are to keep up the strength and to secure sleep. The first is best done by nourishment, and in many cases he thinks an early resort to the feeding tube is the humane and the only justifiable course to pursue. There is no doubt but it might be used to advantage much oftener than it is outside of hospitals. No year passes that he does not see lives saved by its use. A full meal is often a better sleep-compeller than an opiate. Milk, egg, and brandy, with a little nutmeg and sugar, is very palatable and anodyne in its action. As a hypnotic, chloral hydrate is the favourite reliance, in doses of 20–30 grains at bed-time; it will bear to be repeated, but not indefinitely. But in mania all drugs are uncertain in their effect. He holds that insane persons are not to be taken indiscriminately to a hospital simply because they are insane.

A RARE CASE OF RESTITUTION OF THE MEMBRANA TYMPANI AFTER FIFTEEN YEARS OF DISEASE. By C. H. Burnett, M.D., Aural Surgeon to Philadelphia Dispensary and the Presbyterian Hospital.—On the last day of July 1872, Christian L., 15 years old, a German, presented himself for treatment at the Philadelphia Dispensary. His complaint was, that since infancy he has had a discharge from the right ear, with progressive hardness of hearing. He says that there has never been any discharge from the left ear; all of which statements were corroborated by his father, who accompanied him.

My examination revealed the presence of a copious light-green discharge in the meatus. When this was removed I discovered a large perforation in the postero-superior quadrant of the membrana tympani. Hearing-distance for the watch $\frac{1}{2}$ of the norm. Eustachian tube pervious to the current of air from Politzer's apparatus. After cleansing the meatus thoroughly, I introduced ten drops of a strong solution of nitrate of silver, 3j.-f3j., and ordered him to syringe the ear *gently* three times daily, at home, and after each syringing to instil into the meatus ten drops of a solution of sulphate of zinc (gr. ij.-f3j.), allowing them to remain in contact with the affected parts five minutes.

One week later I saw the boy again, and ordered a continuation of the treatment already described, since his condition had greatly improved. By the middle of August the discharge had ceased, and the hearing for the watch had increased to $\frac{1}{2}$ of the norm. On the 19th August the perforation in the membrana tympani had closed, and the membrane, which at the time of the first examination was swollen and discoloured, had assumed the normal shining appearance. The hearing had become relatively normal—i.e., the previously affected ear had assumed a hearing-power equal to that of the unaffected ear, but neither ear was endowed with *sharp* hearing-power, and probably had not been since early childhood. The interesting features of this case are:—1. The length of the duration of the disease. 2. The exquisite restitution of the membrana tympani. 3. The restoration of the hearing. 4. The obvious benefits of the treatment.—*Philadelphia Medical Times*.

LITTLE ON THE USE OF DIGITALIS IN THE FAILING HEART AND DELIRIUM OF ACUTE DISEASES.—Dr Little (*Irish Hospital Gazette* Jan. 15, 1873) recommends the use of digitalis in febrile cases in which stimulants are either not well borne or are contra-indicated, as, for example, in most cases where renal affection is present. He has given it in more than twenty cases, including six of typhus and one of rheumatic fever, the remainder being cases of enteric fever. He usually gives half-drachm doses of the tincture every three or four hours, rarely every hour, and discontinues the medicine when the pulse falls to eighty. Along with the digitalis he gives wine and brandy. In one case of rheumatic fever where stimulants could not be borne, he used tincture of digitalis alone. After several doses nausea occurred. The tincture was therefore discontinued, and a hypodermic injection, composed of one-eightieth of a grain of atropia, one-fortieth of a grain of digitalin, and one-fourth of a grain of morphia, was substituted. The patient eventually recovered.

RESINA COPAIBÆ AS A DIURETIC (*Lancet*, 22d March 1873.)—Dr Wilks speaks with the greatest confidence of the value of the resin of copaiba as a diuretic. The ordinary copaiba has long been known for its action on the kidney, and is occasionally used in

dropsy, but the nauseous taste of the oleo-resin has almost forbidden its employment. The oil separated from the resin is officinal, and is often prescribed in gonorrhœa instead of the compound substance. It is thought that the oil acts more especially on the mucous membranes, and is therefore useful in affections of the bronchial, vesical, and urethral surfaces. If this be so, it is equally certain that the diuretic properties reside in the resin. Dr Wilks has found it very difficult for patients to take the ordinary pharmacopœial drug, and almost impossible to get general practitioners to sanction its administration in private practice: he therefore has substituted for it the simple resin, and finds it equally or more efficacious. He gives fifteen or twenty grains in mucilage and flavoured water three or four times a day, and has had numerous cases showing its marked diuretic properties. There was in Guy's Hospital a man who came in with ascites, and who, after taking numerous other remedies, was ordered the resin. The amount of urine was at once doubled in quantity, and after a few days the fluid almost entirely disappeared. Dr Wilks states he has lately had as a private patient a "drunkard builder," for whom he likewise prescribed the resin; a diuretic action was at once effected, and the dropsy quickly disappeared. In heart cases also he has given it with great success. Lately there was in the hospital a girl with mitral disease and considerable dropsy, who took the usual medicines without effect, and was then ordered the copaiba. It at once produced the desired effect, and the fluid was dispersed. Dr Wilks says he has often given the remedy and failed; but, on the other hand, when it has succeeded, the result has been more striking than with any other diuretic he has seen. He would desire to see it introduced into the pharmacopœia, as it is not kept by chemists, although he is informed that the balsam is used largely by perfumers, and the resin thrown away in large quantities as a waste material.

℞ Resinæ copaibæ, ℥iii.; spt. rectificati, ℥v.; spt. chloroformi, ℥i.; mucilaginis, ℥ij.; aquam fontanam ad ℥xii. Sig., One oz. to be taken three times a day.

NOTES OF PRACTICE AND PECULIARITIES IN TREATMENT IN THE NEW YORK CHARITY HOSPITAL.

Burns.—A lotion recommended for burns consists of—℞ Sulphate of zinc, grs. xv.; co. spts. lavender, ℥i.; aqua, one pint. M.

Erysipelas.—The only successful method of treatment which has been employed for arresting the spread of the disease is the formation of a boundary-line by means of a blister. For this purpose, the vesicating collodion is ordinarily used. If erysipelas attacks a limb, encircle it with a blister about one-half or three-quarters of an inch wide, at a free point above the part affected. A very common, and most excellent, application for the erysipelas, is an ointment composed of—℞ Sulphate of iron, ℥i.; sapo or unguentum ceræ, ℥i.

M. In this way the iron can be used, and the stains avoided, which are so annoying when employed in solution.

Simple Cerate.—A modification of this dressing is used which renders it much more agreeable, and much easier of manipulation in cold weather. It consists in the addition of oil of almonds, in the proportion of 3i. to ʒi. This makes one of those little variations in surgical dressings which may add to the comfort of the patient and the convenience of the surgeon.

Persistent Vomiting.—There was a case of persistent vomiting in connexion with Bright's disease. The patient was a female, and this was the second attack she had suffered. A number of months previous, in her first attack, the vomiting was sufficiently persistent to reject all remedial agents employed for its relief, and it was supposed that the patient must then and there die. She was placed, however, upon treatment by the use of raw beef, or so nearly raw that it could hardly be said to be an infringement upon the proper use of terms to call it raw, and she began immediately to improve. Her recovery was complete so far as the vomiting was concerned. Her second attack had come on only a few days previous to my visit, and she was placed upon the beef treatment at once. The results were equally satisfactory with the first, and the patient was now able to take iced milk with her beef, and was feeling very comfortable. The beef, raw and seasoned with a little salt and pepper, or cooked in the slightest degree over coals and seasoned in the same way, is taken in quantities averaging about one ounce every three hours. It was cut in small pieces, set by the bedside, and the patient administered it "piecemeal."

Sprains.—This class of injuries is placed at once in a plaster-of-Paris splint. Absolute rest and external support are the essentials in treatment.

Subacute Pleurisy.—Tonics are regarded as an essential element in the treatment of this affection (quinine and iron chiefly), and their administration is made the leading feature. The utility of tapping is looked upon as questionable; at all events, it is not to be resorted to early. Diuretics are administered only for the purpose of maintaining the quantity of urine at its normal standard. When a diuretic is required, infusion digitalis is the one commonly employed. Some of the potassa salts are combined with it if not sufficiently active when administered alone.

Ulcers.—A dressing which is said to serve a most admirable purpose for any ulcerated surface which may need a soothing and slightly stimulating application, is the ceratum resinæ et bals. Peruvian. It is usually employed in the proportion of one part of balsam to four of cerate.

How to Remove Adhesive Plaster.—Every surgeon, doubtless, is familiar with the appearance of a part which has been enveloped in adhesive plaster, after the straps have been removed. The appearance is not one in very good keeping with a cleanly and neat surgical

dressing. The portion of the plaster which is left adhering to the skin may be quickly and completely removed by the use of oil of turpentine and sweet oil. Use a little more than half turpentine. This compound, carefully rubbed over the parts with a bit of cloth or sponge, and then washed off with warm soap-suds, will leave the surface as clean as nature ever intended.—*New York Medical Record.*

Part Fifth.

MEDICAL NEWS.

PROFESSOR GAIRDNER'S LECTURE ON CRIMINAL INSANITY.

QUESTION OF RESPONSIBILITY.—In the case of disturbances of the intellect or the emotions, operating on the sane and insane mind, there may no doubt be difficulties in drawing a satisfactory line of demarcation for legal or other purposes in particular cases; but that such disturbances may go far to constitute the evidence of insanity is freely admitted by all. But the question of moral insanity—the *insanity of the conscience*—is in itself environed with immense difficulties; indeed, it has often been virtually contended that disturbances of this nature do not come within the province of disease, otherwise than as all sinful and criminal acts whatever may be spoken of, in a certain sense, as diseased states of the mind.

. . . But we cannot afford, as men living in a state of society, to give too much effect to metaphysical distinctions tending to excuse crime. . . . Accordingly, the very existence of what has been called moral insanity (meaning thereby a lesion of the moral sense alone, apart from other disorders of the mental faculties) has been denied in the courts of law by many of those who, in the discharge of their duty with regard to such cases, feel that its admission is incompatible with the public welfare. . . . There are, however, cases where the power of the will, either to resist wrong-doing, or, generally, to regulate the conduct, is certainly weakened by disease, either hereditary or personal, or both,—where the resulting act is a crime, but where it cannot be said that the reason is disordered to the extent just indicated, nor even that the moral sense is so far affected as not to permit, in the abstract, the knowledge of good and evil. It is in these cases that the law is often felt to be a hard and cruel taskmaster, even by those who are ready to admit that, in the interest of society, crimes must be punished without reference to the subtleties arising out of the consideration of their motives. An illustration of this may be drawn from another kind of experience—viz., that of the school, where, moreover, both the legal and the moral discipline are, to a great extent, placed in the

hands of one and the same authority—the schoolmaster. Every educator is familiar with the case of boys so obtuse, and, at the same time, so obstinate and passionate, as to suggest to every humane mind that there is something wanting or morbid about the mental organization—something that cannot be reached and mastered by mere threatenings, or, indeed, by any amount of actual penal severity, any more than it can be entirely and satisfactorily dealt with according to the law of kindness. Now, in such a case, suppose unlimited power and unswerving resolution to be brought to bear upon a special and determinate issue—a task that *must* be done, an order that *must* be obeyed, cost what it may. . . . We do not require to indulge in speculation in order to foreshadow what must occasionally happen in such a case; for we have it actually before us, in the case of Reginald Channel Cancellor, who was done to death by Thomas Hopley, a schoolmaster at Eastbourne, on the 22d April 1860, under exactly the circumstances above stated. It was proved in evidence, that the poor boy had a positively diseased brain, which rendered him both dull and unmanageable, so that discipline of various kinds and in various schools had been tried with him in vain; it was equally proved in evidence, I think, that under an overwhelming and relentless sense of “duty,” fortified by a commission from the boy’s father to use any severities that might be required, the schoolmaster flogged and punished the boy to death, praying with him in the course of the punishment, and entreating him to surrender, and to do what was required of him. The theory on which this wretched man proceeded was obviously that he had a sane and stubborn will to deal with, which must absolutely be brought to submission before the kindness and moral discipline which were intended to follow could take effect. And the fitting sequel to this horrible tragedy was that, from the prison where he was confined after sentence, there came forth a statement of “facts” by the prisoner, in which, so far from appearing to see his educational theory in any new light, he advisedly maintained it as the basis of a model educational establishment, in which he and his wife were to give the world the benefit of their hard-earned skill and experience. Now, conceive of the case of a child like this poor boy Cancellor, morbid in brain, yet not so morbid as to be distinctively classed as insane—passionate and vindictive, or obstinate, sulky, and morose—imagine such a child, not killed at school, but only hardened and rendered worse by maltreatment, and then drifted into the hard, coarse, brutal life of the slums of a great city. Is it possible in such a case always to make a clear distinction between insanity and moral degradation? . . .

If a vice, owing to circumstance, training, or associations, be settled down in the mind, the act (originally within the control of the will) may get in time to be beyond its control. And if this occur as the result of mere habit, much more is it likely to happen if there be an insane taint—inherited, it may be—in the mind, and if the consciousness of harsh and irritating treatment pursued in

ignorance of the insanity be added as a debasing influence on the moral nature. Homicidal monomania has, in this group of disorders, excited more public attention than any other manifestation of the so-called moral insanity. The alternative before us in such a case is very awful—on the one hand death, on the other a complete acquittal on the plea of insanity; for the law holds that if the person is insane he cannot be tried for the offence, much less be subjected to punishment for it. The forms of impulsive insanity manifesting themselves in tendencies to suicide, incendiarism, and theft, have been held by extreme advocates of the doctrine as all constituting distinct insane impulses, which may exist separately, and absolutely without any other insane manifestation. How can vice leading to crime—insane impulse to crime—be possibly discriminated in all cases? For, on the one hand, law cuts us off from the consideration of the motive by affirming that a crime is a crime even if committed without a motive; on the other, psychology maintains the existence of insane impulses to theft or murder distinguishable only, if at all, through the scrutiny of the motive. It is, in fact, quite impossible. Acknowledging the mystery, we must just regulate our procedure by the principles of practical expediency, and not by the mystifications of metaphysics or law. Acts of murder and other crimes are committed by insane people who, notwithstanding their insanity, are to a certain extent responsible, inasmuch as they can discriminate right and wrong, are conscious of committing offences, and can also be shown to have had a certain power of controlling their impulses. There is not an asylum in the country which could be conducted as asylums, happily, now are conducted, if the actions of the insane were presumed to be removed from the sphere of moral responsibility. Nothing but iron restraint could keep an asylum in order if the power of self-control in the confessedly insane were not acted on as a fact. There can be no absolute distinction between the crimes of the sane and the insane. The degree of criminality may be largely affected by the conditions of the insanity. But in the case of the sane, other conditions (such as bad education, ignorance, weakness of mind, bad associates, the control of others—as of a husband over a wife, or a master over a servant) must also be admitted, and are in fact admitted, by the law, to a certain extent, in judging of the degree of criminality. The insane cannot be put into a class and, as such, labelled morally irresponsible: the question must be tried in each individual case as to its own circumstances. If the existence of insane delusions be established before the occurrence of the crime, and if they be of such a kind as naturally to lead up to the perpetration of the deed, no doubt the loss of responsibility is complete, and the law decides rightly in such a case. But the law does not recognise intermediate grades, where the morbid tendency, although perhaps hereditary, is still compatible with some degree of moral responsibility. When, under certain accidental circumstances, such a person commits a great crime, we cannot refuse to admit, as one fact, a c

degree of responsibility, and, as another fact, a really morbid tendency. According as the experts appointed to examine the prisoner are inclined to lay stress on the one fact or on the other, we have them giving evidence on either side, and so arise the conflicts of opinion in our Courts. But if the law clearly acknowledged the fact of partial, or rather incomplete, responsibility—responsibility up to a certain point, a responsibility not to be estimated by any name appended to the mental disorder, but judged of and settled for each individual case, and allowing of a corresponding modification in regard to punishment—we should then have fewer contradictions in the witness-box on the part of experts; the moral sense of the public would be satisfied by some less degree of punishment than the extreme penalty of the law in many cases, and time would thus be given in cases that remain doubtful for a better and truer appreciation of the real mental condition of the prisoner than is possible during the few critical weeks which precede his trial. The tendency to the creation and perpetuation of groups of special insane perversions of the moral sense (such, *e.g.*, as kleptomania) would cease to have any practical importance if every case had to be tried on its merits with regard to such qualified judgments. An answer, moreover, would thus be found to the indignant questions so frequently put after a prisoner has been acquitted or reprieved on the ground of insanity. Is a man to be regarded as irresponsible, it is often asked, for the crimes he has committed, because his grandfather or his aunt happened to be insane? The answer would be, no; but this fact of insanity in the family should be taken into account in judging of the degree of criminality implied in his offence. This view of the partial responsibility of the insane is supported by the adhesion of Casper, the great German authority on legal medicine; and it is also formally admitted, to the fullest possible extent, in Prussian jurisprudence.

MORISONIAN LECTURES.—On the 20th June, Dr T. S. Clouston, of the Royal Edinburgh Asylum, Morningside, brought these lectures to a successful termination. Before the commencement of his last illness, Dr Skae had been appointed Morisonian Lecturer on Insanity to the Royal College of Physicians, and at his instance Dr Clouston was requested to act as his substitute. Dr Skae, however, only left three lectures ready, and the other three had to be completed by the lecturer himself. Dr Skae's lectures were intended as a practical illustration of his classification of the various modifications of insanity, as originating in physical alterations produced by violence or disease or as modified by them. And had they been concluded and amplified, as was, we believe, the intention of Dr Skae, they would have formed an admirable text-book of mental disease from a physical point of view. Dr Clouston deserves great credit for the admirable manner in which he gave Dr Skae's prepared lectures, and for the very able manner in which he supplemented these lectures and finished the course. The

concluding lecture was chiefly occupied by a most elaborate and interesting account of general paralysis, as it affects what we so curiously term the body and the mind—meaning simply the different nervous centres, in which Dr Clouston pointed out that it was a consideration of this disease that gave the first hint for the adoption of a physical basis of mental disease; and that very important information in this direction was to be obtained from a future careful investigation of the pathology of general paralysis and of epilepsy. At the conclusion of the lecture, Dr Clouston received a unanimous vote of thanks, which was moved by Dr Halliday Douglas, Vice-President of the Royal College of Physicians, and seconded by Dr Arthur Mitchell, one of Her Majesty's Commissioners in Lunacy for Scotland.

THE MEDICAL ACT AMENDMENT BILL.—We noticed in our last that the Royal College of Surgeons had adopted a petition against this bill; since then, the Royal College of Physicians have also adopted a petition against this bill, believing that “no sufficient cause can be shown for superseding the existing examining boards in the performance of the duties entrusted to them by the legislature.” Moreover, the following joint statement has been issued by the two Colleges:—

1. That under the Medical Act 1858, the Medical Council has been for fifteen years engaged in carrying out important practical reform in medical education and examination.

2. That though it has been attempted by vague and *ex parte* statements, in various quarters, to throw discredit on the results of the operation of the Medical Act 1858, yet, that ample evidence can be adduced to show that already it has produced the most beneficial effects as regards the medical profession and the public, and that, if allowed its free course, it is capable of proving still more effective.

3. That in consequence of the recommendations of the Medical Council, the course of professional study exacted by all the licensing boards has been extended and improved; entrance has been barred against the admission of illiterate persons into the medical profession, by the enforcement of an examination in general education previously to the candidate being allowed to commence his medical studies; the professional examinations of all the licensing bodies have been rendered more extensive, more practical, more uniform, more stringent, and consequently more efficient. As a proof that the licensing boards are doing their duty to the public, and are not amenable to the charge of undue leniency in their examinations, it can be shown, by an official table of the results of the examinations for licences, which has been recently issued by the Medical Council, that about *one-third* of the candidates examined failed to satisfy the examiners, and were remitted to their studies.

4. That by Clause XIX. of the Medical Act 1858, provision has been made by which one or more of the licensing bodies (under the sanction of the Medical Council) unite their examina-

together, so as to give by one series of examinations, and at a moderate expense, a complete qualification in all branches of Medicine and Surgery. That that clause has been taken advantage of for a number of years by the Royal Colleges of Physicians and Surgeons of Edinburgh, and the Faculty of Physicians and Surgeons of Glasgow, and that similar arrangements might easily, and with advantage, be carried out in England and Ireland without fresh legislation, except in those cases where the charters of certain examining bodies require revision, in order to give them the necessary power to form such combinations.

5. That the establishment of three licensing boards, viz., one for England, one for Scotland, and one for Ireland (as contemplated in Mr Headlam's Bill), which are to supersede the present licensing boards, and henceforth to exercise the sole licensing power, would be extremely difficult of accomplishment, inconvenient, cumbersome, and attended with great expense. That this would be especially the case in Scotland, where the teaching and licensing bodies are located in cities considerably apart from one another—viz., Edinburgh, Glasgow, Aberdeen, and St Andrews—necessitating either peripatetic boards of examiners—a most inconvenient, troublesome, and expensive arrangement—or that all candidates for licences should be obliged to undergo examination in Edinburgh, which would be to them inconvenient and expensive. Hitherto the fees paid by candidates to the licensing bodies in Scotland have been moderate in amount, a complete qualification in Medicine and Surgery being attainable for sixteen guineas. Under the system proposed in Mr Headlam's Bill, it is calculated that it will be necessary for candidates to pay thirty guineas, or nearly double. This would form a heavy tax on the poorer students, of whom, in Scotland, the number is very considerable.

6. That the wants of the country, as regards the supply of general medical practitioners, are better served by the licences being granted as at present by the licensing bodies included in Schedule A to the Medical Act, 1858 (always, of course, under due inspection and supervision by the Medical Council), than they would be under one board for each division of the kingdom (as proposed in Mr Headlam's Bill), in which universities and medical corporations are mixed together—since there would be thereby a risk, on the one hand, that the standard of competency being raised too high, and the expense increased, the country might lack its due supply of general medical practitioners, especially in the remote, extensive, and far from wealthy districts of Scotland—and, on the other hand, a risk that the standard of university degrees, which ought undoubtedly to be higher than that of the diplomas of general practitioners, might be lowered, and that thereby the supply of more highly educated physicians might be diminished. The uniformity of examination aimed at in Mr Headlam's Bill would have a tendency to lead to uniformity of teaching, and thereby cramp the development of new views, and thus tend to produce a dead level, as

teachers would be compelled to adhere to a system of teaching such as would fit their pupils for passing the examinations of the new boards.

7. That due provision has been made in the Medical Act, 1858, for the visitation and inspection of the examinations of the licensing boards by the Medical Council, or by persons deputed by it for the purpose; that already this has been carried out, to some extent, with the most beneficial effects, and that a more systematically organized method of inspection, which was resolved upon by the Medical Council during its late session, and which is now about to be put in execution, promises to be quite effective for every good purpose.

8. That the continually maintained agitation, in certain quarters, for medical reform tends to create uncertainty, and to retard, rather than advance, the progress of the profession, by suspending the adoption by the Medical Council of improvements which might otherwise be pushed on.

9. That the passing of Mr Headlam's Bill would, by depriving the medical authorities of the licensing powers which they have so long exercised, lower their status, cripple their resources, and thus interfere with an important organization of the profession, the destruction of which would be a serious public loss.

10. That fresh medical legislation of the kind proposed in Mr Headlam's Bill thus seems to be uncalled for at the present time; but that, under any circumstances, if there is to be a medical bill, it should be preceded by a fair and full inquiry, either by a select committee of the House of Commons, or by a Royal Commission, so as by competent and reliable testimony to ascertain the real facts of the case, and thus obtain a sure basis for any legislative enactments which such investigation might show to be desirable.

ARMY MEDICAL WARRANT.—The Royal College of Surgeons have also adopted and transmitted to the Director-General of the Army Medical Department, a memorial in reference to the Army Medical Warrant of 1st March 1873, deprecating it as in many respects unjust and injurious to the medical officers of the army.

THE NEWEST THING IN LARYNGOSCOPY is the introduction by Schrötter of metallic bougies for the treatment of stricture of the glottis. These are worn day and night, and are fastened both by the mouth and by the canula. An account of his cases will probably be published.

A CORRESPONDENT informs us that the French Government, unlike our own, are fully alive to the importance of scientific investigations, and have given Ranvier a laboratory for histology and vivisection at the Collège de France, which offers unusual facilities for the prosecution of histological and pathological research. Already many of the Parisian Hospital Internes are to be found working steadily there at pathology and morbid anatomy; and a school of microscopy seems likely to arise, which will compete with the Prussians more seriously than Marshal Leboeuf did.

DIFFERENCE OF LONGITUDE.—"We are sorry," says the *London Medical Times and Gazette*, "to observe that the medical officer of St George's-in-the-East Workhouse, against the unanimous wish of the Guardians, is strenuously opposing what is in most work-houses a customary allowance of beer for paupers who work as tailors, shoemakers, etc., thus partially recouping the parish for their keep. It is difficult to conceive what valid objection the doctor can have against the very moderate allowance of beer customary in such cases. However, as the Poor Law Orders require the sanction of the medical officer to the bestowal of malt liquors, we suppose the working inmates will be deprived of their harmless luxury."

Paupers in this country are not accustomed, except by special orders of the physician, to such "harmless luxuries."—*Medical Times, Philadelphia.*

OBITUARY.—Full of years and of honours, MORITZ HEINRICH ROMBERG died on the 16th ult. at Berlin, of cardiac disease, under which he had laboured for some years, and the symptoms of which had become more serious during the last three months. Romberg was born at Meiningen on the 11th of November 1795, and was consequently in his 78th year at his death. He very early in his professional life devoted himself to the study of nervous diseases as the great object of his life, and his first publication on this subject was a translation, with annotations, of Marshall's "Anatomy of the Brain in Mania." The researches of Sir Charles Bell filled him with enthusiasm, and he also translated this great work in 1831 for the use of his German professional brethren. With the exception of an edition of Albertini's "Opuscula Medica," which he edited, a couple of Reports on Cholera, in which he had a large experience, and some fugitive articles in medical journals, all Dr Romberg's publications had relation to his favourite subject, and may be said to have culminated in his celebrated "Lehrbuch der Nervenkrankheiten des Menschen," which was published in three parts in 1840, '43, and '46, and has passed through three editions, the last of which was published in 1857. This work was translated into English for the Sydenham Society by Dr Sieveking in 1853. Thus the name of Romberg has now long been familiar to medical inquirers, and his memory deserves to be held in special respect by all British medical men;—first, for the upright honesty of his character, which led him always to acknowledge literary and scientific obligations where they were due; second, for the peculiarly practical character of all his works; third, for the strenuous manner in which he warned against a tendency, even more prevalent now, to base the study of pathological phenomena on the results obtained in the test-tube, or by the scalpel and microscope, to the exclusion of the only true guiding principle to be found in physiology alone; and last, not least, for the strong predilection which he has always shown for the physiologists and pathologists of Britain.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*A Case of Intrauterine Polypus.* By J. MATTHEWS DUNCAN, M.D., and KRONID SLAVJANSKY, M.D., St Petersburg.

(Read before the Obstetrical Society of Edinburgh, 11th June.)

THE following case occurred recently in my practice, and I narrate it not so much on account of its own interest, as on account of the importance attaching to the investigation of the structure and nature of the tumour by Dr Slavjansky. While I am responsible for the brief clinical history of the case, he is responsible for the anatomical investigation hereafter to be given, and for the conclusions derived therefrom.

Miss E., æt. 52, has enjoyed almost uninterrupted good health up till the commencement of her present illness two years ago. Now she is very weak, pale, and of anæmic appearance. She complains of pain in the back, but chiefly in the lower part of the abdomen, in which situation it is greatly aggravated at times, and especially during the night.

Two years ago, she began to be troubled with a copious, red, watery discharge. After it had endured for nine months, she began to have, in addition, discharges of clotted blood from the vagina, and occasionally of pieces which she describes as fleshy. The discharge still continues, and it is not fetid.

The hymen was found entire, and was ruptured by passing the finger to examine the uterus. This organ was found to be enlarged and retroverted, and had a globular form, as felt through the roof of the vagina: it was not tender. There was no sign of the existence of perimetric adhesions. The cervix was enlarged, being distended; and its os was open to such an extent as to admit the tip of the examining index finger. The finger, pressed against the os, discovers a greatly enlarged cervical cavity, high in which the surface of a tumour or polypus can be reached.

Continuing my investigation on a subsequent day, I enlarged the os uteri by scissors, cutting through its lip, and passed my finger into the uterine cavity; while my left hand, applied over the liv-

pogastrium, pressed the uterus down upon the examining finger of the right hand. I then discovered, occupying the upper parts of the cervical cavity, a rounded tumour, of about $1\frac{1}{2}$ inch in diameter, having a smooth surface, diversified with smooth, soft projections of about the size of a split pea. The neck of this tumour I judged to be little less than an inch in diameter: it was surrounded by a constriction apparently formed by the internal os uteri, and passed upwards to its insertion in the fundus uteri. While what I regarded as the cavity of the cervix was greatly distended by the tumour, which also did not quite fill it; the cavity of the body of the uterus was short, and occupied by the thick pedicle of the polypoid tumour.

I immediately proceeded to remove it, and found that it was very fragile, so that it could not be seized, far less removed in one mass. Under the finger and forceps it broke down into unnumbered pieces, many of which were washed out of the vagina by a lotion used after the operation was over. Ultimately, the whole mass and its neck were removed down to the level of the wall of the fundus uteri. I then was able to feel, on the wall of the uterus, close to the insertion of the pedicle of the tumour, some soft projections about the size of a split pea.

The patient has done well, and has returned to her home in the country. There is still discharge going on, but less in amount than formerly.

The clinical history of this case as recorded—including the age of the patient, the nature of the discharge, the softness and fragility of the polypus, the existence of small sessile masses at the insertion of the pedicle, the degradation of the general health—has led me to be very cautious in prognosis as to the result of the operation of removal of the tumour. I can entertain very little doubt that, whether the disease is as yet truly malignant or not, it will, before many months are passed, show the terrible characters of undoubted cancer.

Anatomical Investigation. By Kronid Slavjansky, M.D.

I received from Dr Matthews Duncan several fragments of the above-described tumour. The largest of these was about $\frac{3}{4}$ inch long and $\frac{1}{2}$ inch broad. It was of very soft consistence, and the fresh cut surface yielded a considerable quantity of juice slightly tinged with blood. On the cut surface there could be easily seen many extravasations of blood, which were of different ages, being of different colours and consistence; the older being somewhat brown and more solidified than the more recent, which were softer, red, and quite freshly coagulated.

Microscopical examination of the juice showed many red and white blood-corpuscles, cylindrical epithelial cells, and fusiform cells of the character of connective tissue. Besides, there were seen many cells of different sizes, some having two or three nuclei and a quite

transparent protoplasm, which becomes opaque when treated with acetic acid, the opacity not disappearing in excess of acetic acid.

Before sections were made, I hardened the tumour by the following method:—Quite fresh pieces were immersed in a solution of the following composition: two parts of bichromate of potass, one part of sulphate of soda, one hundred parts of water (Müller's solution), and allowed to remain in it for a week. Afterwards they were taken out and placed in pure alcohol till they became hard enough to slice. Then slices were made with a razor, and subjected to microscopical inspection. By the lower power (see Fig. 1), the tumour is seen to be permeated by many canals of different sizes and shapes, between which lies common slightly fibrillar connective tissue, in which are observed many extravasations.

The higher power (see Fig. 2) shows that the canals are lined with a continuous stratum of epithelium, the cells of which have the character of short cylindrical, with a well-marked nucleus. On the inner surface of the epithelial layer are often observed quite round cells of a very transparent aspect. These cells can be found also lying in the lumina of the canals conglomerated in masses. The size of the cells is a little greater than that of a white blood-corpuscle: sometimes the nucleus can be detected: the reaction of the transparent protoplasm is that of a mucus. Besides these cells, we find in the canals enlarged epithelial cells whose protoplasm has also become transparent. In a few places such altered epithelial cells can be found lying *in situ* in the lining epithelial membrane of the canals.

The demarcation between the epithelial layer and the connective tissue is, in every part of the tumour, extremely well marked. The connective tissue consists of a very few fine fibrils and fusiform cells, and in many places it is infiltrated with wandering white cells. This connective tissue, as it lies between the canals, is in some parts reduced to a small line consisting of only one layer of fusiform cells; but in other parts it is very thick, and contains bloodvessels, which present, for the most part, the appearance of veins, very few having the characters of arteries. The capillaries are in some places very highly developed, and can be seen filled with blood-corpuscles.

In many parts of the tumour can be found large extravasations, which have, in these parts, very much destroyed the structure of the tumour, so that its tissues are broken up, and lie in the midst of the blood, and these show sometimes traces of fatty degeneration.

Extravasated blood can also be found lying free in the canals, and sometimes in such abundance as to obstruct them.

From this description, it is evident that we have here an organoid tumour, the structure of which is quite homologous with that of the mucous membrane of the uterus. The canals, which are lined by short cylindrical epithelium, and in transverse sections

have more or less rounded forms, are the hyperplastic representative of the uterine glands. The character of the connective tissue between them is quite the same as in the normal mucous membrane. Considering this structure, and the absence of any deviation in arrangement of the epithelium and connective tissue, the tumour must be placed in the category of polypous hyperplasia of the mucous membrane; and keeping in mind the actual polypoid form observed clinically, and the presence of many extravasations in the tissue, we must name this tumour *adenoma polyposum hæmorrhagicum uteri*.

Investigation with the naked eye discovered some signs—for example, softness and presence of juice—which might induce a belief in the malignant nature of the tumour, but the microscopic examination gives a sufficient explanation of the different kinds of cell elements in the juice without supposing malignancy. The presence of the epithelial element, analogous to the epithelial cells of the uterine glands, is well explained by the easy separability of them from the underlying connective tissue. The mucous degenerated cells, as we know from the microscopic investigation, may be derived from two sources: the largest of them, occasionally containing nuclei, are degenerated epithelial cells; and the smallest are degenerated wandering white cells, such as are observed¹ in the sputa from the bronchi. It is thus seen that the microscope affords no distinct evidence of the malignant character of the fragments of the tumour which I received.

In recent times attention has been directed² to a special anatomical sign of carcinoma,—namely, that the mutual arrangement of the epithelium and connective tissue is disturbed, and the epithelium is found lying free between the fibres of the connective tissue. Complete absence of this anatomical sign proves that this tumour is not a cancerous one.

EXPLANATION OF THE PLATE.

Fig. 1. Transverse section of the tumour, showing oblique transverse and longitudinal sections of uterine glands, the lumina of many of which are filled with extravasated blood and mucus corpuscles. Hartnack; ocular 4 and lens 3.

Fig. 2. The same seen by a higher power, showing the relation between the epithelium and the connective tissue. On the internal surface of the epithelium at two parts, the projecting mucus corpuscles. Hartnack; ocular 4 and lens 7.

Both preparations are coloured with ammoniacal solution of carmine.

¹ See Slavjansky, Virchow's Archiv, Band 48, 1869. *Experimentelle Beiträge zur Pneumonokoniosis-Lehre*.

² Rudneff, On the Development of Carcinoma. *Journal for Pathological Histology*. St Petersburg, 1871.



Fig. 1

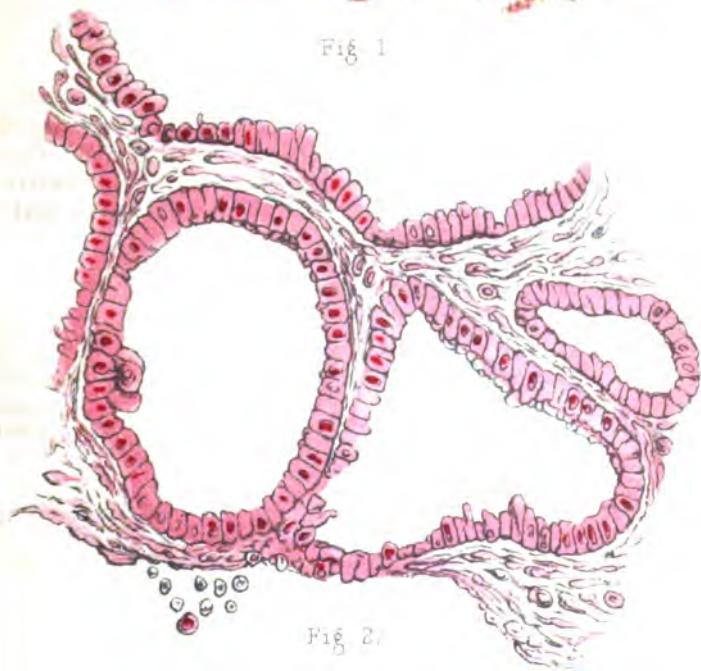
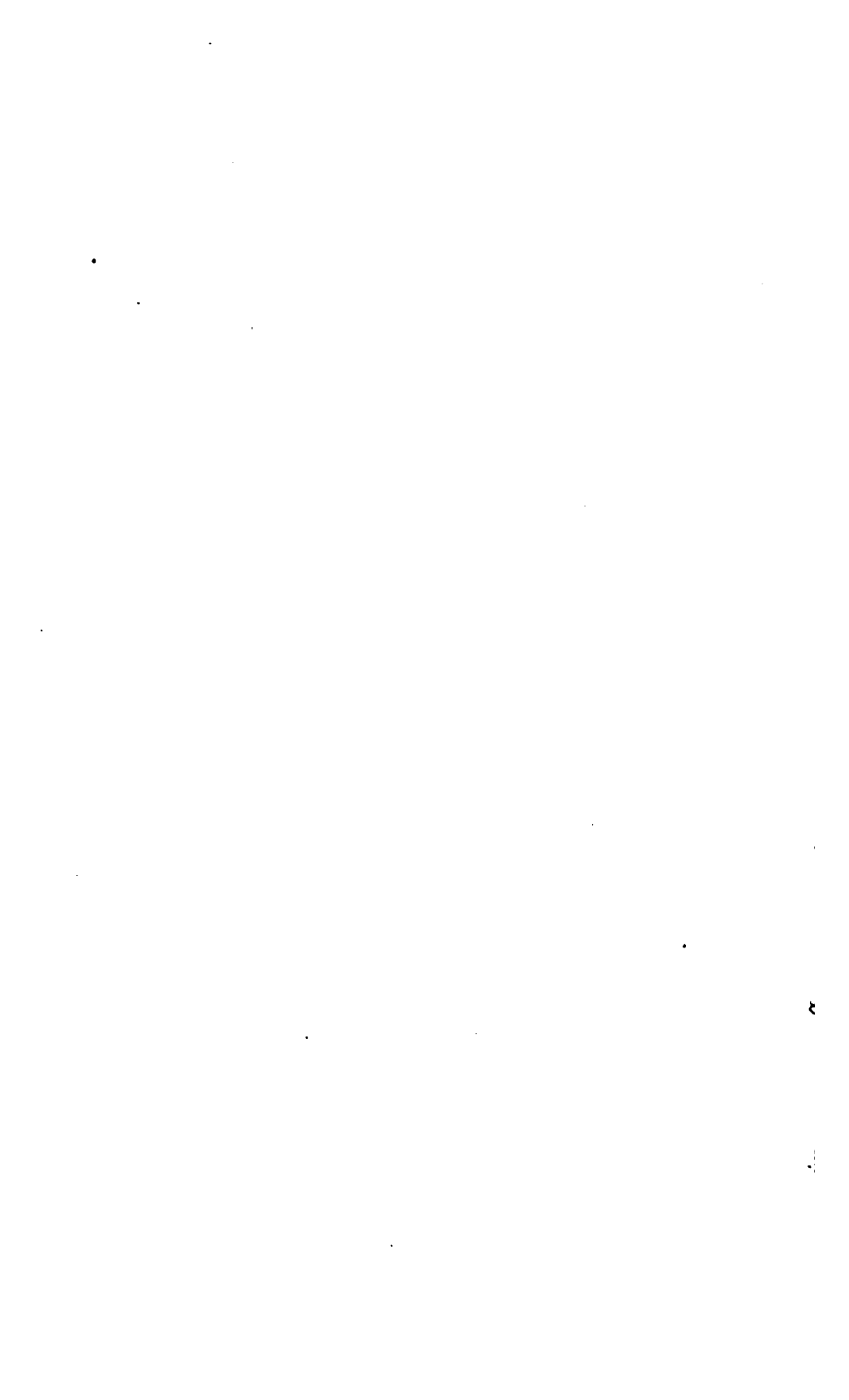


Fig. 2.



ARTICLE II.—*On Darwinism in its Relation to the Higher Faculties of Man.* By J. W. EASTWOOD, M.D. Edin., M.R.C.P. Lond., Dinsdale Park Retreat, Darlington.

(Read before the Medico-Chirurgical Society of Edinburgh, 19th March.)

IT is with considerable hesitation that I bring before the Society such a subject as this for discussion. As it is too wide to deal with fairly in one paper, or even in a series of papers, I have preferred taking only some portions of such a large inquiry, and particularly those portions which are most interesting to me as a psychologist and ethnologist. Darwin's views ought to have great interest in Edinburgh, where so many men are devoted to medical, scientific, and philosophic pursuits. Some years ago, Professor Huxley lectured at the Philosophical Institution on "The Relation of Man to the Lower Animals," and one of the greatest ornaments of our University, Professor Goodsir, opposed the growing views of Darwin, which were advocated in the lecture. It is my intention to steer entirely clear of Revealed Religion, and to treat the author's views on his own grounds.

In Mr Darwin's work on "The Descent of Man," the object, he states, is principally to ascertain:—

1. Whether man, like every other species, is descended from some pre-existing form.
2. The manner of his development.
3. The value of the differences between the so-called races of man.

The author admits the high antiquity of man, and that there is a considerable difference between him and the higher apes. He states that the conclusion, that "man is the co-descendant with other species of some ancient, lower, and extinct form, is not in any degree new;" that Lamarck long ago held this opinion, and that at the present time it is supported by such men as Huxley, Wallace, Lyell, Vogt, Lubbock, Häckel, etc. The doctrines of evolution and natural selection are strongly and ably contended for by Darwin. It is impossible not to admit the vast labour and extensive researches of this philosopher in the field of natural history, which he has made more attractive than it was before. His facts and observations are numerous, and wonderfully arranged; but it appears to me that he has allowed them to engross so much of his attention, that he has been unable to take a wide view of his subject. The facts which are opposed to his views are not fairly brought forward; and it is necessary to state at the outset of this paper, that he has not carried conviction to my mind that he is correct in his conclusions. I may here remark, that I do not oppose the doctrine of natural selection *per se*, for within certain limits it is no doubt true; but to elevate it into the chief power by which the lowest germ becomes man, seems to be unreasonable.

In the first chapter, on "The Evidence of the Descent of Man from

some lower form," the development of the bodily structure is chiefly considered, and on this part of the subject I shall be very brief. The author states his views in the following passage:—"It is notorious that man is constructed on the same general type or model with other mammals. All the bones in his skeleton can be compared with corresponding bones in a monkey, a bat, or seal. So it is with his muscles, nerves, bloodvessels, and internal viscera. The brain, the most important of all the organs, follows the same law, as shown by Huxley and other anatomists." Now I do not see how any one, Darwinite or anti-Darwinite, can take any objection to this passage. I believe, too, that the bodily structure of the mammalia, at least, is constructed on the same general type or model; but this is very different from believing that each animal in the scale of creation is descended from the next lower in the series. If we adopt this view, we must go not only to the lowest of the animal creation, but pass on to the vegetable kingdom, and say that man is descended from the lowest vegetable germ, and that there has been no separate creation of any animal or vegetable. Now, in plain terms, this means that the ultimate vegetable atom had conferred upon itself the power, by a process of evolution and development, to become the intellectual, moral, and spiritual being called man. Such a wonderful act of creative power requires more faith than to believe that various plants and animals were separately created for the various purposes which they fulfil—at least within certain limits. It appears useless to discuss the question whether this ultimate atom had, *inherent in itself*, the power to become man. Such a thing is contrary to all our ordinary experience, for everything around us shows that things have been made by some intelligent mind, either the Creator or man, to subserve certain definite purposes. I assert that the general type or model existed *in the mind of the Creator*, and on this type or model, with an infinite variety of modifications, by the influence of surrounding circumstances, and by the aid of natural selection, everything in creation, animate and inanimate, was made. If the original atom had the power in and by itself of becoming man, how came it to possess that power? If the atom created itself with such vast powers of evolution, then nature created itself, and the mind of man repels such an idea. I cannot better illustrate the subject than by showing you a photograph of the first railway engine ever made, which now stands as a monument to the genius of George Stephenson at one of the railway stations at Darlington. I have many times examined it, and have observed how simple it is in its machinery, how coarse its structure, and yet how well adapted for its purpose, which it fulfilled for many years, that of running as a locomotive between Darlington and Stockton, on the first railway ever made. This engine is the parent, so to speak, of the very perfect and complicated engines of the present day. The evolution, however, which has taken place has been in the minds of the *creators*

or *designers* of the engines, and not owing to any power of evolution contained in the engine itself. The type or model is here, and on this type each engineer who has made improvements has been continually working. And this engine is only the successor of more primitive engines, evolved from them in the brains of the discoverers. I am aware that the engine is an inanimate object, and so far the argument may not hold good, but it sufficiently illustrates my meaning.

The paragraph quoted from Mr Darwin's work may therefore be admitted as generally true; but when we make use of analogy, the weight of this argument is materially lessened as regards the brain. The muscles of the ape and of man do not differ very greatly from each other, and they subserve to a great extent the same purpose. They are formed on the same plan, and, considering the different habits of these two highest specimens of the mammalia, they are wonderfully alike. So are the brains, especially of the young. This resemblance proves too much, for, according to this view, the apes should be much nearer to man than we actually find them to be. There ought not to be such a wide gulf between them as that which exists, as regards their position as intellectual animals. The resemblance between them does not to my mind explain the difference in the higher attributes, and the probability of man's descent from the quadrumana, arguing from bodily structure alone, is rendered more difficult to establish. The difference is *other than this*, and must be taken on higher grounds, which it is my especial object to bring forward.

After mentioning a great number of instances of similarity in the structure of man and the other mammalia, some of which are obvious, and many are very remarkable, Mr Darwin concludes his first chapter by the following decided summary of his views:—

"We can understand how it has come to pass that man and all other vertebrate animals have been constructed on the same general model, why they pass through the same early stages of development, and why they retain certain rudiments in common. Consequently he ought frankly to admit their community of descent: to take any other view is to admit that our own structure, and that of all animals around us, is a mere snare to entrap our judgment. This conclusion is greatly strengthened if we look to the members of the whole animal series, and consider the evidence derived from their affinities or classification, their geographical distribution, and geological succession. It is only our natural prejudice, and that arrogance which made our forefathers declare that they were descended from demigods, which leads us to demur to this conclusion. But the time will come when it will be thought wonderful that naturalists, who were well acquainted with the comparative structure and development of man and other mammals, should have believed that each was the work of a separate act of creation."

This conclusion might have been assented to had man been

only a higher ape ; but as I contend that, intellectually, morally, and spiritually, he is something so much higher, the dogmatic conclusion of the author loses its weight, for he ignores to some extent man's higher attributes. In judging of such a subject as this, we must take into consideration every argument. In opposition to the views just quoted, I would place those of a very profound anatomist, Professor Goodsir, who states, "that the human body presents a whole series of perfected arrangements of structure, bearing immediately on the higher conscious or rational principle of man—arrangements which are deficient in all apes alike, and which thus collectively, by their absence, distinguish all the apes from man corporeally, as precisely as their instinctive form of consciousness separates them from man psychically." And again, "Man, in virtue of his possession of a spiritual principle, by which alone he is capable of thought and speech, and is impressed with the belief of moral truth and divine agency, *stands alone* among the organized beings of this globe."

There is a further argument to be derived from the comparison of the bodily structure of man and the apes. Man is the master of all animals only by virtue of his superior intelligence, which gives him the possession of instruments or firearms. But this is by no means the case with the next series to man. The gorilla is a dreadful brute, and physically man's master. The chimpanzee is a mild animal, and has no authority over other animals by reason of its superior mental organization, or by physical strength. The author admits that though man in his bodily structure shows a close resemblance to the ape, which no one is disposed to deny, yet that in mental faculties there is an enormous difference between the lowest savage, who can only count five on his fingers, and the highest ape. He points out that there is a greater difference between some of the lowest animals and the ape, which difference is gradually bridged over, than between the ape and man, in respect of mental faculties ; and hence concludes that there is no fundamental difference between them, but only one of degree. On this subject he is evidently beyond his depth, and he does not attempt to explain how it is that, whilst the difference between the higher apes and man in bodily structure is actually less than between the apes and the next step in the scale, yet that there should be such an immense distance between man and the quadrumana in the higher attributes. This may be summed up in the possession of the highest reason, self-consciousness, language, moral powers, and spiritual feelings ; in other words, whilst the physical structures of man and the higher apes bear such a close resemblance, there is such a wide gulf between them in the psychical and other attributes. It needs a more profound anatomist than Mr Darwin to harmonize these conflicting facts, and this part of his work is highly unsatisfactory. It is when the author descends to details that he is interesting and instructive. He proceeds to notice the resemblance

between the mental faculties of the mammalia generally, and brings forward a number of curious instances showing this resemblance in instincts, emotions, the power of imitation, memory, etc. And because dogs, cats, and horses have vivid dreams, "we must admit," he says, "that they possess some power of imagination!" And because a monkey has been found in a state of nature to crack a nut with a stone, the author considers him capable of using tools! There is an immense difference between this power of using tools and making a steam-engine or constructing a telegraph. As to language, he confesses, "that the fact of the higher apes not using their vocal organs for speech depends on their intelligence not having been sufficiently advanced." In other words, the apes are but apes, and they can do no more than the lower monkeys. The greatest living authority on the subject of language, Professor Max Müller, maintains that the absence from the vocal expressions of the lower animals of anything like a phonetic type or verbal root was of itself sufficient to set aside the view that human language is but a development of the inarticulate sounds of brutes. It is his conviction that language is the great line of demarcation between man and the lower animals.

With respect to Natural Religion, Mr Darwin says, that "the belief in God has been advanced as not only the greatest but most complete of all the distinctions between man and the lower animals. It is, however, impossible, as we have seen, to maintain that this belief is innate or instinctive in man." In support of this view, the author can only bring forward the fact that in some very degraded nations or tribes there is no idea of God and of a future state. The absence of this belief, however, occurs only in some few barbarous tribes, reduced by physical deprivations and other causes to a lower level than their neighbours; just as in our own country we find our street arabs and criminal population almost entirely destitute of moral and religious ideas. But in Africa, a native savage can argue skilfully with a Moffat, a Livingstone, and a Colenso, respecting the highest of all subjects—man's future state. This shows that the inferiority mentioned is local and not general—that it is the product of evident causes; for that without civilisation, in the ordinary acceptance of the term, man has a distinct belief in a superior being and in a future life. We have evidence, too, of this in the earliest historical periods of man's history, for on the tombs of Egypt we find it plainly recorded that more than 4000 years ago the belief in a God and a future life was quite distinct and well established. The condition of the so-called lowest races of man is an important one in this argument; and respecting one of these, the aborigines of Australia, I have lately obtained some interesting information. An official report from New South Wales has been presented by the Earl of Kimberley to the Anthropological Institute. One observer writes thus:—"In every part of Australia where I have conversed with the aborigines, they have a traditional

belief in one Supreme Creator. It seems strange to those who are at all familiar with the thoughts of these people, that in such standard works as the 'Encyclopædia Britannica,' the statement should be repeated, even in late editions, that the aborigines of Australia have no notion of any beings superior to themselves. The fact is, they believe in many unseen spirits, some benevolent and some mischievous, departed spirits of dead men, and demons of forest, lake, river, and mountain, and they also believe in one Supreme God." In this respect, these much-despised people are quite on a level with some of the most cultivated nations of antiquity, as the Persians, Greeks, and Romans. "If asked who made the sky, the earth, the animals, and man, they always answer, 'Baiāme.' Some avow the belief that when good men die their souls go up to heaven to Baiāme, while the bad, when they die, cease to exist. Some say that all, good and bad, go up to heaven." . . . "The Milky Way is called Worambul, a watercouse, with a grove abounding in food, flowers, fruit, and all that is desirable. To this Worambul the souls of the good ascend when their bodies are committed to the grave, and they are supposed to be cognisant to some extent of what takes place on earth, and even to have the power to help their fellow-men below when invoked." Another observer, Mr Günther, says, as the result of several years' converse with these people:—"There is no doubt in my mind that the name Baiāme refers to the Supreme Being; and the ideas concerning Him by some of the more thoughtful aborigines are a remnant of original traditions prevalent among the ancients about the Deity." He also found "traces of three attributes of the God of the Bible—viz., eternity, omnipotence, and goodness." Another author, a colonial magistrate, says, that "the natives of the Loddon river have a tradition of a being possessing some of the attributes of supreme power, to whom they assign the creation of the first man; the name of this being is Binbeal. He made all things. He subjects the spirits of deceased persons to the ordeal of fire, to try them whether good or bad; the good being at once liberated, while the bad are left to suffer for an indeterminate period. They also imagine that the souls of some persons pass into the bodies of certain beasts." I need scarcely remind you how old a doctrine this is, and of its existence amongst some of the most civilized of the ancient inhabitants of Europe and Asia.

And now, a few words as to the languages of these people, who are fast disappearing before the advance of the white man. They are of a complicated character, with singular, dual, and plural numbers, like the ancient Greek, and in many other respects resembling that complex and highly-organized language. They have affixes, prefixes, and inflections which convey great niceties of meaning, such as are utterly unknown in the English language. And one dialect, called the Northumberland, which Mr Threlkeld examined carefully, and elaborately illustrated,—in the abundant

affixes, the minute modifications of the verbs and pronouns, and other forms, has its counterparts in all the Australian languages. There are many remarkable resemblances between them and the Aryan languages, especially in the formation of cases by inflexion, like the Latin and Greek.

It would be easy to bring forward evidence from other parts of the world—from Africa, through Livingstone and others, where European civilisation has never travelled, and from the American Indians—to show that the lowest races of mankind are in possession of certain beliefs, and use a language so complex, that they are very far removed from the highest quadrumana. And, further, abundant evidence may be obtained to prove that these people, in their present state, could not have originated these complex languages, but that they had been derived from people in a higher state of civilisation or mental culture. This view is quite opposed to Mr Darwin's theory, and this is my excuse for entering into these particulars. In holding his views, it is necessary to admit that man has attained his present position from a *lower one*. The history of man, however, within certain limits, is both progressive and retrogressive; and many savage nations retain traces of what their more cultivated ancestors believed and learnt. That the Australian is a connecting link between the higher races of man and the apes—a statement frequently made—is not supported by any reliable evidence. In a work called "Sketches in Western Australia," a lady writes thus:—"Granting that the lowest condition of mankind is to be found on the great island-continent, I can yet assure Europeans that they have no reason to feel ashamed of owning affinity with the savages of Australia West, either in respect of mental qualities or that of manly appearance. The kangaroo mantle, nearly reaching to the knee, hangs gracefully over their fine figures; the uncovered head is carried loftily, and a dignity is added to the high, well-shaped forehead by the binding of a fillet round the hair and brow, after the fashion of an antique bust."

I may here make a general complaint against Darwin, that he bends his facts to serve his theories, and I will give one striking instance of this. In the first volume of his work, he is advancing arguments to show the unity of the human race, and he says that when at Terra del Fuego, whilst at first he was struck with the savage appearance of the inhabitants, yet when they were on board, dressed, and had learnt some words of English, he was surprised to see how closely they resembled Europeans, not only in physical appearance, but in disposition and most of the mental faculties, so that there could be no doubt about the general brotherhood of man. At the conclusion of the second volume, he again alludes to these Fuegians, and I quote the passage as being so opposite to what he had before written. "The astonishment," he says, "which I felt on first seeing a party of Fuegians on a wild and broken shore will never be forgotten by me; for the reflection at once rushed into

mind, Such were our ancestors. These men were absolutely naked and bedaubed with paint, their long hair was tangled, their mouths frothed with excitement, and their expression was wild, startled, and distrustful. They possessed hardly any arts, and, like wild animals, lived on what they could catch; they had no government, and were merciless to every one not of their own small tribe. He who has seen a savage in his native land will not feel much shame, if forced to acknowledge that the blood of some more humble creature flows in his veins. For my own part, I would as soon be descended from that heroic little monkey, who braved his dreaded enemy, in order to save the life of his keeper; or from that old baboon, who, descending from the mountains, carried away in triumph his young comrade from a crowd of astonished dogs,—as from a savage who delights to torture his enemies, offers up bloody sacrifices, practises infanticide without remorse, treats his wives like slaves, knows no decency, and is haunted by the grossest superstitions." Surely we need not go out of our own country to find persons so degraded as to practise most of the above list of savage enormities.

The author is of opinion that the moral sense or conscience is the most important of all the differences between man and the lower animals. He thinks the latter show love and sympathy, and other qualities called moral. The instances he gives are of a very inferior kind. There are some animals which perform actions from instinct, having the appearance of being moral, but which are not to be classed with the proper moral actions of man. Notwithstanding that the author thinks the moral sense is the highest attribute of man, as he considers the belief in God is not universal, though I have shown it to exist in the lowest races of mankind, he concludes that there is only a difference of degree and not of kind between the faculties of man and the apes. He makes the same mistake as others have made, who are not physiologists, with regard to some of the so-called mental or moral actions of animals, which are due to reflex action instead of to the higher powers.

In the chapter on the development of man from some lower form, Mr Darwin says "that as all animals tend to multiply beyond their means of subsistence, so it must have been with the progenitors of man; and this will inevitably have led to a struggle for existence and to natural selection." And again, "In regard to bodily size or strength, we do not know whether man is descended from some comparatively small species, like the chimpanzee, or from one as powerful as the gorilla; and therefore we cannot say whether man has become larger and stronger, or smaller and weaker, in comparison with his progenitors." To those who are readily disposed to take up new theories, it may be a matter of no difficulty whatever to believe that in the same continent, and in the same country, the principle of natural selection should have produced such different animals as the elephant and the tiger, the chimpanzee and the gorilla, surrounded by exactly the same physical conditions.

These two last animals inhabit either the same or contiguous districts of west central Africa. Physically different as they are, the chimpanzee maintains its own existence. As we cannot suppose for a moment that the gorilla is incapable of defending itself in the struggle for existence, we may well ask, Where are its present descendants, supposing man to have descended from the chimpanzee, which Mr Darwin considers probable, rather than from the gorilla?—at least, he says, “it might have been an advantage to man to have sprung from some comparatively weak creature.” In this part of the argument, it is evident that the author considers that there are some series of animals between the quadrumana and man which he calls the progenitors of man. It is here that some of the greatest difficulties are to be met with. No fossil remains having been found of any animals between the two highest classes, although the remains of other recent animals have been often found which were probably coexistent with the supposed missing links, we must keep our attention fixed chiefly upon the higher apes. We will exclude all but the chimpanzee, the oran outang, and the gorilla. From which of them is man descended? And how comes it to pass that these animals should be so unlike, whilst their habits, mode of progression, and general characteristics, are still so similar? The gorilla is a mere brute, and the young, as De Chaillu found, is absolutely untamable. It seems further removed from man, in its higher attributes, than the devoted companion of man, the dog. There is no man-like ape in America, yet the Red Indian is surely equal to the African negro or the native of Borneo. There is no analogy whatever in the comparison between the so-called progenitors of man and the races of men by which they are surrounded. Mr Darwin naïvely but truly admits, that “undoubtedly it would have been very interesting to have traced the development of each separate faculty, from the state in which it exists in the lower animals to that in which it exists in man; but neither my ability nor my knowledge permit the attempt.” He thinks that the early state of man was a barbarous one, and it is in accordance with his views that not only the series which has resulted in man, but man himself, has been gradually progressive. This is a favourite view with many persons, but it cannot be maintained; for though modern times have shown certain races to excel in material welfare, and in some of the arts and sciences, yet we have still to go back thousands of years for perfection in many beautiful arts, for great advances in philosophy, in mathematics, in poetry and kindred subjects. The builders of the temples of Egypt, of Assyria, and of Central America, were not so much behind our modern architects; and in writings, our greatest living statesman goes back to Homer as to an inexhaustible fountain. And besides these, we have the writers of the Old Testament, so full of rich poetic imagery and spiritual feeling, not excelled by any modern writers. If ancient man were so barbarous, whence come such

elaborate forms of language as the Sanscrit, the Arabic, the Hebrew, and the Greek, as well as the languages of now barbarous nations?

We have evidence of another kind to prove that, long before the historic period, man was then equal to the existing man. The latest found fossil human remains were in the caverns at Mentone, on the Riviera, and the description by Dr Henry Bennett shows that the fossil man bore "no resemblance whatever to the orang outang or to any monkey." The skull was found to be elongated, very convex superiorly, dolico-cephalic. The structure was similar to that of other pre-historic men, but different from modern man. The skeleton was above six feet in height, nineteen feet below the surface of the cave, in a recumbent position, and along with it were found the remains of the hyena, several species of *ursus*, *felis*, and *canis*, the rhinoceros, and some extinct animals. The fauna was long since extinct. There is here no evidence of inferiority, and as remains of man have been found, why not those of the extinct progenitors of man?

In the chapter on the affinities and genealogy of man, Mr Darwin refers to the classification of Blumenbach and Cuvier, who placed man in a separate order. Huxley thinks, referring to organization, that there is "no justification for placing them in a distinct order." He therefore divides this order into three sub-orders, the *Anthropidæ*, the *Simiadæ*, and the *Lemuridæ*. To this I have no objection, if man is to be considered merely as an animal; but an anatomist like Huxley ought to take into consideration all those other qualities, mental, moral, and spiritual, which are as much a part of his nature and organization as his arms or his legs. Good-sir opposed this view of man's nature, which has become so prevalent in the present day. Both Huxley and Darwin thus, as it were, ignore a part of man's nature, and can see only certain details of organization, which prevent them from making those large generalizations and accurate deductions which belong to true philosophers. On this account, therefore, I consider the weight of Huxley's opinions as materially lessened in such an argument as this.

The *Simiadæ* are divided into two groups—the *Catarrhine* or Old-World monkeys, and the *Platyrrhine* or New-World group; and Mr Darwin thinks man is descended from the former; that his birthplace was in the Old World, not in Australia or any of the islands, but probably in Africa, where the gorilla and the chimpanzee exist. But why not in Borneo, where the orang outang lives? for formerly Borneo was connected with a great southern continent. The author is evidently aware of the great difficulties in this part of his subject, for he says:—"The great break in the organic chain between man and his nearest allies, which cannot be bridged over by any extinct or living species, has often been advanced as a grave objection to the belief that man is descended from some lower form; but this objection will not appear of much weight to those who, convinced by general reasons, believe in the

general principle of evolution." Certainly, if we accept the doctrine, no objections are of any consequence. And again he says, "In attempting to trace the genealogy of the mammalia, and therefore of man, lower down in the series, we become involved in greater and greater obscurity."

In bringing this imperfect paper to a conclusion, it is scarcely necessary to state how difficult a task the author feels that he has set himself in proving, what he thinks conclusively, that the doctrine of evolution is the only one by which we can explain the origin of man. The chief difficulties are undoubtedly connected with man's intellectual, moral, and spiritual faculties. I contend that the difference between man and the highest apes, as to bodily structure, is comparatively *small*, and they are truly kindred animals; so that Huxley has actually bridged over the gulf, by placing them in the same natural order; but the difference in all the higher attributes is entirely out of proportion to the physical structure. In intellect the highest mammalia, the apes included, are stationary, whilst man is progressive. Many of the mental operations of the lower animals are mere instinct. In moral faculties the difference is greater still, not explained by our present knowledge of their brains. To a certain extent, the higher mammals are conscious; man is self-conscious; and there is scarcely a trace in them of the high moral attributes of man. That man has also lower qualities proves nothing to the contrary, and only shows that he has capacities for good and for evil. In spiritual attributes there is no comparison at all; for the author himself is obliged to admit that the belief in God is the highest quality of civilized man, and that it has exercised the most beneficial influence on his conduct. He concludes his work by saying that "man still bears in his frame the indelible stamp of his lowly origin." To this I cordially agree, and would add, that man bears also in his mental, moral, and spiritual nature the indelible stamp of his divine origin.

ARTICLE III.—*On the Treatment of Glandular Affections.* By F. PAGE ATKINSON, M.D., etc., late Surgeon to St Bartholomew's Hospital, Chatham, and Royal South London Dispensary.

ALTHOUGH we are unable by medical treatment to lessen the duration of the various septic fevers, it is quite in our power, I believe, to cut short the progress of inflammatory disease; and I think that everything which upon long-continued experience seems to effect this purpose, should be placed clearly before the profession, in order that it may receive a full and impartial trial. I consider that we have at our command all the means necessary for the cure or relief of disease, and that it is simply owing to our imperfect knowledge as regards the actions of drugs, or the true nature of the

various ailments presented to us, that they are not duly and rightly administered. I have often been astonished at the uncertainty which seems to prevail in the treatment of glandular affections, especially those of the breast and tonsils. In the first of those mentioned, cold and evaporating lotions are recommended as local applications by some, and warm poultices by others. In the latter, the inhalation of steam is advocated by some, and the sucking of ice by others. Of course it may be argued that cold and heat bring about the same results by different means; but the question is, whether either method of treatment is, truly speaking, beneficial; whether the progress of the disease is, in reality, in any way altered,—whether, in fact, it is shortened in duration or not. The same diversity of opinion exists with respect to the internal as to the external treatment of these diseases; but it must be admitted by every one that such ought not to be the case. The fact is, we have all been apt to trust too much to the knowledge that has been handed down to us from our forefathers, instead of inquiring for ourselves as to the rationale, the why and the wherefore of what we do, and the consequence is, that medicine has not made the progress it otherwise might have done.

Acute glandular inflammation, speaking generally, and according to my own experience, requires the administration internally of the effervescent citrate of potash, and the application locally of a sedative, or the tincture of iodine. The citrate of potash and the bicarbonate (into which the citrate afterwards becomes changed) may both be described as saline febrifuges. They are alteratives, especially to the glandular system, and give activity to the secreting and excreting organs. They also influence the composition of the blood by rendering the fibrin less plastic. Citric acid by itself is said to diminish the preternatural heat of the system, and to allay thirst and irritation of the skin.

But this outline of treatment, brief though I wish to be, will scarcely explain all that I have to say on the present occasion; and I shall therefore show what requires to be done in each particular instance. First, then, as regards *Quinsy*. In this disease, I can predict with certainty that any patient will be quite well, and able to resume his duties, on the fourth day; whereas, by the old method of treatment, the disease lasted from nine to ten days. I do not know of a single instance in which matter has formed, except prior to the time of the patient coming under my care. The prescriptions I give are the following:—

20 grains of bicarbonate of potash; 30 minims of the compound tincture of guaiacum; as much as is necessary of the compound tragacanth powder—in one ounce of water; and 15 grains of citric acid, in half an ounce of water. To be taken in a state of effervescence, three or four times daily.

25 minims of the tincture of iodine, in an ounce of water, to be used as a gargle three or four times daily; three or four glasses of

port wine in the course of the twenty-four hours, and as much beef-tea as the patient can take.

The throat should be left uncovered, and poultices, steam inhalations, etc., should be particularly avoided, as also should the use of purgatives. In these cases there is generally a rheumatic tendency; and it will be found on inquiry that there has been excessive mental or bodily exertion prior to the attack.

Quinsy is not the result of cold; for, if it were, laryngitis would be a more frequent accompaniment than it now is. As regards the treatment, I would remark that it must be carried out in its entirety, or the results expected will not be obtained. When supuration has already commenced, order simply the iodine gargle, the port wine and beef-tea, and omit all internal medicines.

In the case of *Inflammation of the Breast*, give the following: 20 grains of bicarbonate of potash; 10 minims of spirits of nitrous ether; 10 minims of aromatic spirit of ammonia—in one ounce of water; and 15 grains of citric acid in half an ounce of water; and order to be taken, in a state of effervescence, every four hours.

Apply to the breast an ointment consisting of three parts of the extract of belladonna, and one of iodine ointment. Keep the patient up with good, strong beef-tea, and if there is much fever, with a quick pulse, give port wine. The rationale of the treatment proposed is this: The effervescing citrate of potash, as stated above, acts as a febrifuge; the nitre relaxes the cutaneous vessels, and lessens the quantity of fluid which keeps flowing to the breast; while the belladonna soothes pain; and the iodine helps the absorption of the lymph which has been thrown out. Where abscess has already occurred, give 30 minims of the perchloride of mercury solution, 15 minims of spirits of chloroform, 15 minims of dilute hydrochloric acid, 60 minims of compound tincture of bark, in one ounce of water, three times daily; and paint the breast with a solution of nitrate of silver (2 grains to the ounce of water).

I have rarely found it necessary to strap the breast, except when the abscess has been very deep, and the opening has taken place on the upper surface of the breast; and even in these cases strapping rarely proves of much service.

In cases of *Inflammation of the Testis*, I order the effervescing citrate of potash, in combination with drachm doses of hyoscyamus. The testicle itself should be well supported, and kept covered with some lint dipped in a lotion of 15 minims of the tincture of opium and 15 minims of the tincture of belladonna to the ounce of water, and this, again, enveloped in oiled silk. This method of treatment will be found to lessen pain, and also the tendency to bubo. When the testicle becomes chronically enlarged, cover it with lint smeared over with blue ointment, and strap, and give the perchloride of mercury and bark internally. Where *Bubo* occurs by itself, give the effervescing citrate of potash and hyoscyamus internally; paint the enlarged gland with iodine, keep it covered with spongiopiline

dipped in a solution of sulphate of zinc and alum (3 grains of each to the ounce of water), and enjoin rest. In both these cases, stimulants should be avoided, and the patient should only take a light diet. Barley-water may be recommended as a drink. Beef-tea, of course, should be freely given. Where the *parotid* becomes inflamed, give the effervescent citrate of potash and guaiacum, paint the gland with tincture of iodine, and then, when dry, apply a linseed-meal poultice which has been made up with a warm lotion, consisting of 3 grains of alum and 3 grains of sulphate of zinc, in one ounce of decoction of poppies. Port wine should be given according to the necessity of the case, and plenty of beef-tea.

Where there is *inflammation of the absorbents*, I order the effervescent citrate of potash and ammonia, and keep the limb encased in a poultice made up as above. When there is *suppuration*, I find it best to prescribe 3 grains of muriate of cinchonine, 15 minims of the tincture of the perchloride of iron, and 15 minims of spirits of chloroform, in one ounce of water, three times daily; port wine or brandy, according to the requirement; and beef-tea, as much as can be taken.

In the case of *Scrofulous Enlargement of the Glands*, give the syrup of the iodide of iron internally, with small doses of gray powder and powdered ipecacuanha, and paint externally with tincture of iodine; and the same treatment may be applied both internally and externally where there is an ulcerated surface. The local application of iodine certainly seems to effect more good than the nitrate of silver.

Where there is *Enlargement of the Thyroid*, apply a lotion constantly, consisting of 3 grains of alum, 3 grains of sulphate of zinc, 3 grains of sulphate of iron, to the ounce of water; and give internally the following mixture:—3 grains of the bromide of potassium, 60 minims of Parrish's chemical food, 10 minims of tincture of digitalis, water to the ounce—three times daily. Pancreatic emulsion is also of use in giving nourishment to the nervous system. Underdone meat, and plenty of farinaceous food, should be also recommended.

There are two chronic diseases of the tonsil which are often overlooked, viz., fatty enlargement, and adenoid disease. According to Mr Harvey, the aurist, these are not unfrequently the cause of deafness, and it is important therefore to bear this in mind on all occasions. In the case of fatty disease of the tonsil, the patient must be put on the same line of diet as is recommended for general obesity. Where there *adenoid disease* of the tonsil, a bougie should be passed along the floor of the nostrils into the pharynx, and after this a little tannate of glycerine in water should be thrown into the pharynx from the nose. The throat should be gargled with iodine, and iodide of potassium should be taken internally.

This, then, is a brief outline of the treatment I have pursued for some years in the case of glandular affections. The results obtained, I think I may say, have been satisfactory, and I hope those who feel inclined to put upon trial what I have suggested, will not be disappointed in their expectations.

ARTICLE IV.—*On Ether and Chloroform in London Practice.* By
CHARLES KIDD, M.D., M.R.C.S. Eng., London.

A CURIOUS clamour or *ex parte* agitation against Edinburgh colleges and chloroform has prevailed in London and Dublin. One or two Americans have, with an allowable national pride, set up ether as the one thing necessary to save all medical men from ruin; they would abolish chloroform by special Act of Parliament, and, as done in America, expel any surgeon from an hospital who would use chloroform in place of the American anæsthetic.

The *Edinburgh Monthly* is always a pleasing resource, or final appeal or reference, of the studious medical man, especially in London. We have done all we could here to crush various things in the hospital archives of the big city. Every step in ovariectomy has been a dire pilgrimage or battle, marked here and there with bits of rags, or peace-offerings or oblations, to the atrocious hobgoblin of college or journalistic routine, with here and there the bones of the dead victims, too, scattered in the way. So is it with chloroform; so has it been with iridectomy; so with our wordy war on tubercle. On the Edinburgh School we rely rather for safer ideas and opinions, away from local passions of London societies.

The public ask, Is ether entirely what is represented? Is it quite trustworthy, free altogether from risk, as so often said? There is much that is incongruous at present in anæsthetics. Is ether always sure—not more cumbrous than chloroform—free from unpleasant after-effects—always to be depended on? Martyrs for the truth—Morton and Wells—are they undeservedly forgotten? and what do we know now to make chloroform not the most terrible and deadly of all poisons? Societies ask, too, or discuss what as to the manner or mode of accident under the several anæsthetics. Small questions no doubt, not as large and learned as those on tubercle.

As supplementing former remarks on ether and chloroform as anæsthetics, the medico-legal bearings of chloroform (*ante*, July 1868), I think it would do good to elicit inquiry on both or all sides of this very serious ether question. Coroners and magistrates are now inclined to help this noise, by encouraging verdicts of malpraxis in all cases. An undue clamour exists against chloroform, and all belonging to the Scotch method of its too free administration. Confusion also not a little obtains, as long expected, as to whether the pulse or the respiration is more important, or whether special bag-inhalers are the “be all and end all” of safe anæsthetics, in preference to the Scottish plainer methods of administration.

To help out this weak agitation, it is shown that, by some American speciality, ether is far preferable; and to men who are ready to advertise the speciality of some American hair-wash, some sewing-machine, or substitute for tea, that chloroform is terri-

homicidal, and with an apparatus special, ether, as in America, for what they know, has not had one death; and in France, at Lyons, the same magic halo of perfect safety has surrounded it: both these latter statements are altogether misleading and erroneous.

The well-known seven deaths at Lyons, and forty-one of the Boston Committee, all from ether, are, from insufficient knowledge, forgotten or misrepresented. The best mode of reasoning in clinical dissertation like this is perhaps by comparing, as J. Stuart Mill would say, groups of facts—a group of like cases of pneumonia, to wit, treated by bleeding with low diet, and a group by absence of these (with generous food, milk, beef-tea, etc.). Thus I have found, of 123 deaths tabulated from chloroform: in one group, more than 85 per cent. were with small doses, in trivial surgical operations on fingers, toes, eyes, teeth, etc., where in the excitement stage the patients have rigid muscles leading to apnoea; no deaths in the group of ovariectomy, large capital operations, and midwifery, in fuller doses.

I find, on looking down the table, that in almost all the last fifty deaths the active Sylvester method was chiefly relied on as a means of resuscitation, with no apparent benefit. In very many of the deaths, too—in nearly all the late ones—it is noted that the "Clover apparatus" had been the one chosen in the case, and used with exceptional skill and watchfulness, yet not of the least value in keeping off fatal accidents. Then, as to age, there are practically no deaths in children; and as to sex, of one return by the Registrar-General of 70 deaths, notwithstanding its extensive use in midwifery, there were but 15 females and 55 males. I think we could double all these numbers now for 300 deaths, and with like result.

Convinced that the danger of chloroform is in *its earlier* action, and that the preliminary stage of excitement under ether is shorter and not so dangerous in this earlier stage, I have for many years been glad to suppose ether was safer than chloroform, and stated in my book it was so; but one list of 41 deaths from ether, another of 36 by Sabarth, have shaken one's faith in its entire safety. It cannot be adopted with safety where candles are flickering about, or the actual cautery required in an operation, as it readily takes fire; it spoils, too, very quickly. Ether is inadmissible where there is hæmorrhage, especially in midwifery; it is very tedious in eye surgery, and attended with much vomiting and struggle. On the other hand, ether is cheaper, more easily procured, the deaths not so sudden. These latter, no doubt, are good points in its favour, and by avoiding hæmorrhage cases, it is an excellent anæsthetic.

We have witnessed generalities and angry nothings for months and years on quite as small questions as this,—viz., whether iridectomy for certain eye affections gave relief; one party affirming the negative, the other the positive. So shall we talk of resections as "mountebank" surgery, or is ovariectomy to be looked on either as a dark crime or a brilliant triumph, coruscating according to those

who adopt it? Is not this latter written in the book of Peaslee with sad and graphic touches?

The recent death of Liebig has brought out all the overpowering routine in more than its usual vigour, but with one accord they seem to miss the fine teaching of the Munich philosopher. One writer, in a leading London morning paper read by the million, says, in its usual gushing manner: Great, ay, true greatness indeed, "lovingly he worked on the plan of Bacon; how he must have loved and been guided by the ever-golden fruitage of the *Novum Organon*;" but what is the fact? Great, ay, truly great, remarks a medical echo, "almost with the instinct of English greatness in all true methods from Aristotle to Bacon, and thence to Comte." What is the actual fact? That Liebig never stopped inveighing against hasty generalizations in England.

The medical student has lost a great model. The great teacher and preacher of the deductive method in medical reasoning; his Essay the backbone of the one great Address of the late Prince Consort (Aberdeen). All his life did Liebig labour to show the utter glitter or sham of the Bacon philosophy in the physiological sciences, especially as talked about and around in English orations, Harveian or Hunterian. His most delightful book, the pages uncut in the London libraries, is that where Liebig, as a study for his class, gives this final decision. "The *Novum Organon* is a barren wilderness, painted theatre scenery, which will not stand the honest light of day and the open air."

Hasty generalization as to cardiac syncope has given a special mark to all London literature of anæsthetics. As blood coagulates by escape of ammonia, we were told, and could not be a vital change, so the chlorine group of anæsthetics was a bad group, no matter, respiration, age, nature of operation, lung capacity, sex, temperament, emotion, etc., and the accident could not be physiological or vital, and alone by study of better chemical conditions could we obtain a universally safe anæsthetic: law the first. Next, as fatty heart is a supposed fact, and a true *causa vera*, it is legitimate to suppose it exists in every case till the contrary is shown.

Thirdly, in this serious synthesis of disease, it is of the *avoiidupois* nature of figures, bearing in mind the law of Pythagoras and fatty heart, that there shall be numerically one death in every numerical two thousand administrations; no matter what is done as to resuscitation or its improvement, no matter how we may strive to unriddle this phrase, "cardiac syncope" (neither syncope nor cardiac, as half admitted by Snow himself, and only offered as a provisional explanation of the cause of death till a better one is obtained); no matter, that better idea we owe to Dr Brown-Sequard, adopted without acknowledgment recently in the *Lancet*.

Practical men in hospital are now more and more satisfied that all this is unsatisfactory. I have never had a death from chloroform, nor have I seen one actually occur in some ten thousand administra

tions; but I am certain I have rescued from death over twenty patients in operating theatres by correct ideas of resuscitation. Medical men believe more and more with the Edinburgh School, that the *condition of the respiration is of more import than the pulse or fear of fatty heart*, and that if the respiration is good, the passage of blood through the heart chambers is assured; if not, the circulation or heart within a short interval is *regularized* by attending to the breathing and lung capacity.

And we most assuredly have instances more than enough of hasty induction in the present subject, notably this one of fatty heart, the Dublin contention being that as fatty heart was supposed to exist in one or two cases by Snow (and we have now about 400 deaths from chloroform), so, till the contrary is shown, our numerical law would lead to the belief it exists in all; and therefore, as a corollary, a complex balloon inhaler for chloroform vapour, or the substitute of ether, will alone save us; much of this founded-on error.

Let us apply this to the pneumonia deaths, and say likewise that in pneumonia by bleeding there is a fixed numerical law, the synthesis of disease requires it, and only can it vary as persons are not bled enough. We do not require to go out of that groove; change of type in disease explains all the rest.

And as to fatty heart, till the contrary is shown, or the entire freedom of ether from death accidents too,—till the contrary is shown it is convenient, but not wise, when the contrary is shown, to suppress it, and help to raise a confusion or controversy on other issues more popular in new “medical records.”

If in 100 deaths we find 85 in trivial operations in the excitement stage, with rigid muscles leading to apnoea, we may be almost certain that these are not mere coincidences. Dr Snow, in his later years, believed there was more danger “in strong and robust persons;” such will struggle and become rigid, while those, as I term it, acclimatized to hospital, hoping for the chloroform with joy, reduced to a state of extreme debility, will bear it better. Holding the breath, as in the second stage, leads to engorgement of right side of heart, till the respiration is restored,—was also one of the observant remarks of this able anæsthetist; but he failed to see the bearing of this fact on resuscitation methods, denied now or lost sight of in the Sylvester method.

Generalities, angry nothings, contradictions as to laws, as to the chlorine group of anæsthetics, fatty heart, nature of figures by which there must be one death in every fixed number of administrations, and therefore by induction all further inquiry is useless, while the effect of temperament, emotion, nature of surgical case to be operated on, idiosyncrasy, age,—is all set aside.

In one of the last debates in Edinburgh (1871), the approved percentages in London and complex inhalers were shown to be delusions, while Dr G. Balfour enunciated the principle that the right side of the heart is the weak point. This was ridiculed in a leading article in the London organ of the Association, and Balfour

had to explain that he was misrepresented.¹ This would-be doubt or ridicule still continues; cardiac syncope, watching of the pulse, complex inhalers, electricity to the heart or par vagum, hold their ground instead.

Only where argument fails is the severer logic of wit or ridicule allowable. Argument may fail as to some hypothesis: Lord Bacon, with all his book-learning, denied the rotation of the earth, in love with the Ptolemaic system or hypothesis; but the wit of after days has smiled away his wigged judgments. Like a lawyer he treated Nature, says Liebig, only on one side, but learnedly.

No doubt, ether has some advantages. It must be judged, however, in a calm unbiassed way, and tested by larger and larger experience and groups of facts. The mode of death by it is slow, by comatose intoxication, not by sudden spasm or trachelismus. It must be judged without clamour or hasty induction,—as carbolic acid, antiseptic treatment, iridectomy, catgut ligatures, and the like: not by making a hobgoblin of chloroform; it must be judged with a risk of deaths little inferior to that of chloroform—not by suppressing such deaths because shallow paraphrasts know nothing about them.

ARTICLE V.—*Surgical Cases in relation to Temperature.* By JOSEPH BELL, F.R.C.S.Ed.; Surgeon to the Royal Infirmary.

(Read before the Medico-Chirurgical Society of Edinburgh, 21st May.)

"It is only of late—only with the aid of exact thermometric observations—that the facts (of increased temperature in certain conditions) have been scientifically established; and hitherto the subject has been much less studied in surgical practice than in connexion with infected fevers and visceral inflammations; especially the connexion of these thermometric facts with others of a chemical kind, is even yet but in progress of being demonstrated." These words are from the pen of Mr Simon, "*Holmes's Surgery*," vol. i.; and since they were written very little has been done by surgeons in this country with the thermometer, though physicians have been most laborious and successful in their researches regarding it.

In the following short paper I wish to make a contribution to the study of this interesting subject, and, at the same time, to relate briefly a few cases which have occurred in my practice, which seem to me of interest in themselves.

Source of Information.—The cases are twenty-five in number, nearly all in hospital. I have selected these on one principle only—i.e., that the thermometric observations are tolerably continuous and full, and that they are beyond suspicion as to their accuracy.

I have discarded about double the number of good operation

¹ The letter referred to will be found at page 544 of the *British Medical Journal* for May 1871.

cases, either because the thermometric observations were not sufficiently prolonged, or because I had doubts of the accuracy and care of the observer. Thus, one very interesting case in which the observations were numerous, I found the nurse's observations at night were always the same as my previous one in the forenoon. This was explained by the fact that she put the thermometer in upside down. The cases have all occurred within the last twelve months.

I am thankful to say that my collection of high temperatures with a fatal issue is very small—only one operation case in hospital having died in that time, and only one in private, whose case was invalidated by incompleteness in recording.

I have no new or strange doctrines to propound, no revolution in practice to suggest, but I have found the careful record of temperature of great value to myself in prognosis and treatment, and wish to report what it teaches me in one or two axioms.

1. *Suppuration*, even very profuse, does not necessarily imply any great rise in temperature, so long as it is not putrid.

2. *Fœtor* or putrefaction of suppuration *always* induces a rise in temperature.

3. A high temperature, lasting for more than three or four days after the injury or operation, indicates mischief impending, such as sloughing or abscess.

4. The temperature generally gives warning a day, or even two days, before the pulse.

To illustrate these points, I have divided my cases into three sets:—

- | | |
|--|----|
| 1. Cases of recovery from operation or injury without putrefaction of suppuration, ... | 13 |
| 2. Cases where temperature warned of the approach of mischief, ... | 8 |
| 3. Cases of fatal issue with high temperature, ... | 4 |

I.—*Cases of Recovery from Operation without Putrefaction of Suppuration.*

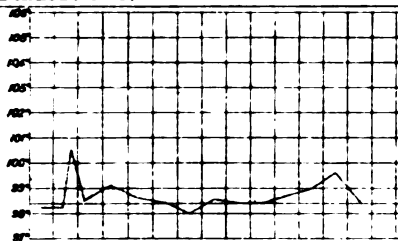
1. *Excision of Mamma*.—Mrs B., æt. 39. Breast excised on 20th Aug. 1872. Nearly whole wound healed at once, except a small portion where the skin had to be removed. Except on day of operation, her temperature never exceeded 99·6°, and that only once; all other readings were below 99° (Fig. 1).¹

2. *Amputation at Ankle*.—A. B., 16, a very delicate girl, had her foot amputated at ankle for strumous disease. It was kept sweet from the first, healed rapidly, and she was out of bed on sixteenth day. Her temperature never touched 100°, except immediately after operation, though her pulse varied from 100 to 120 (Fig. 2).

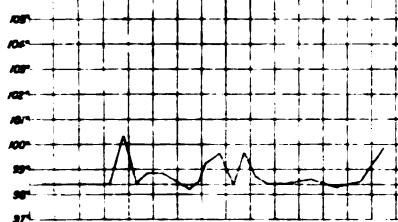
3. *Excision of Breast and Glands*.—J. G., æt. 40. Extensive scirrhus of right mamma and axillary glands. I removed

¹ The figures refer to the lithographs.

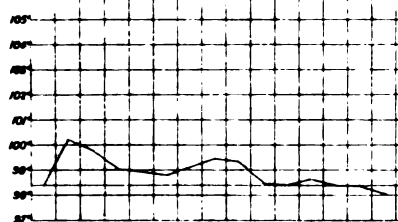
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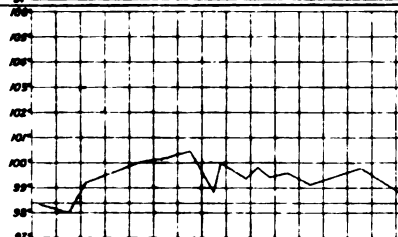
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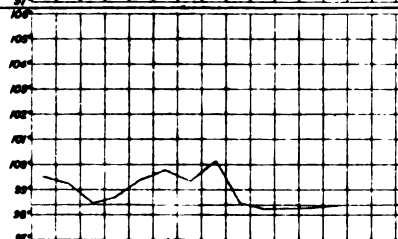
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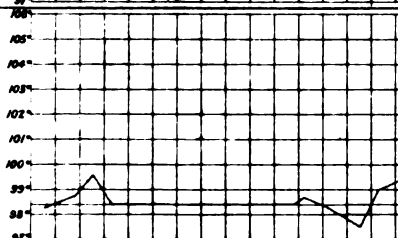
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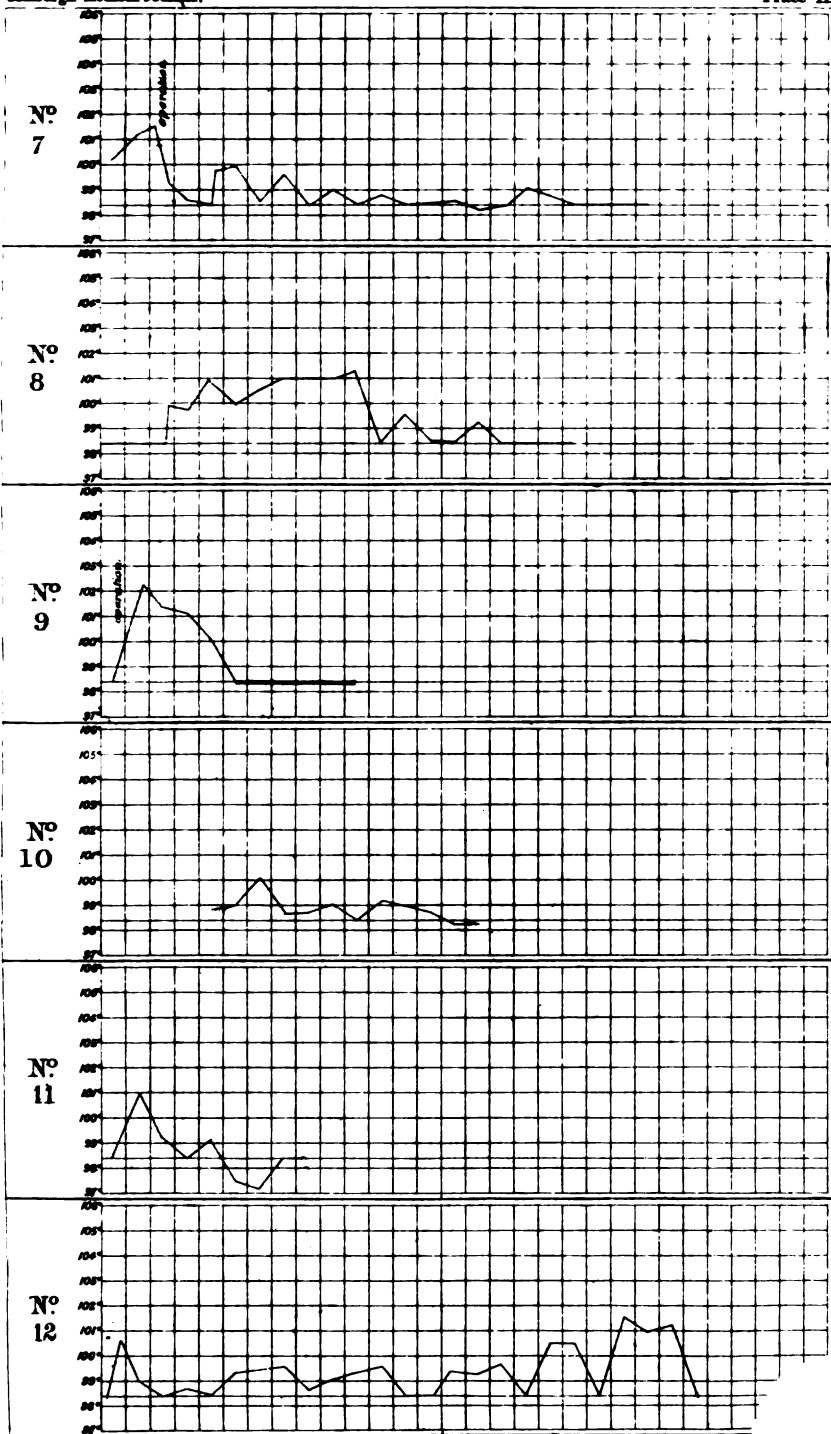


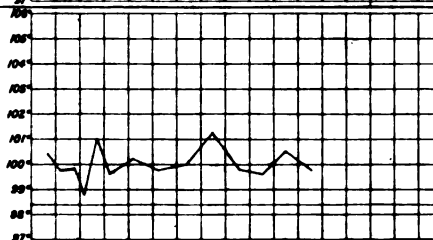
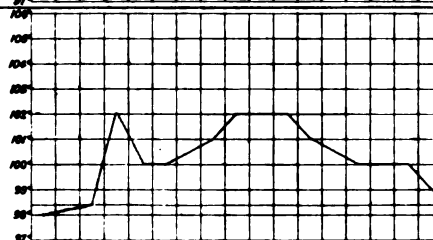
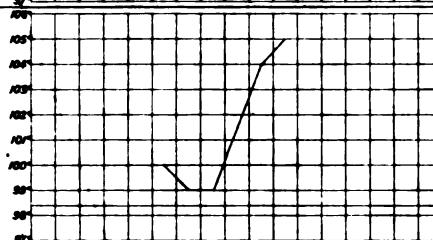
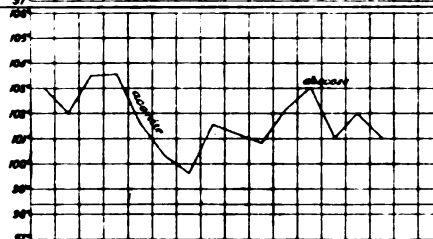
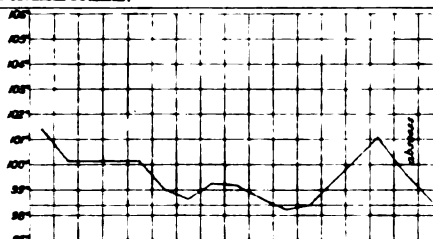
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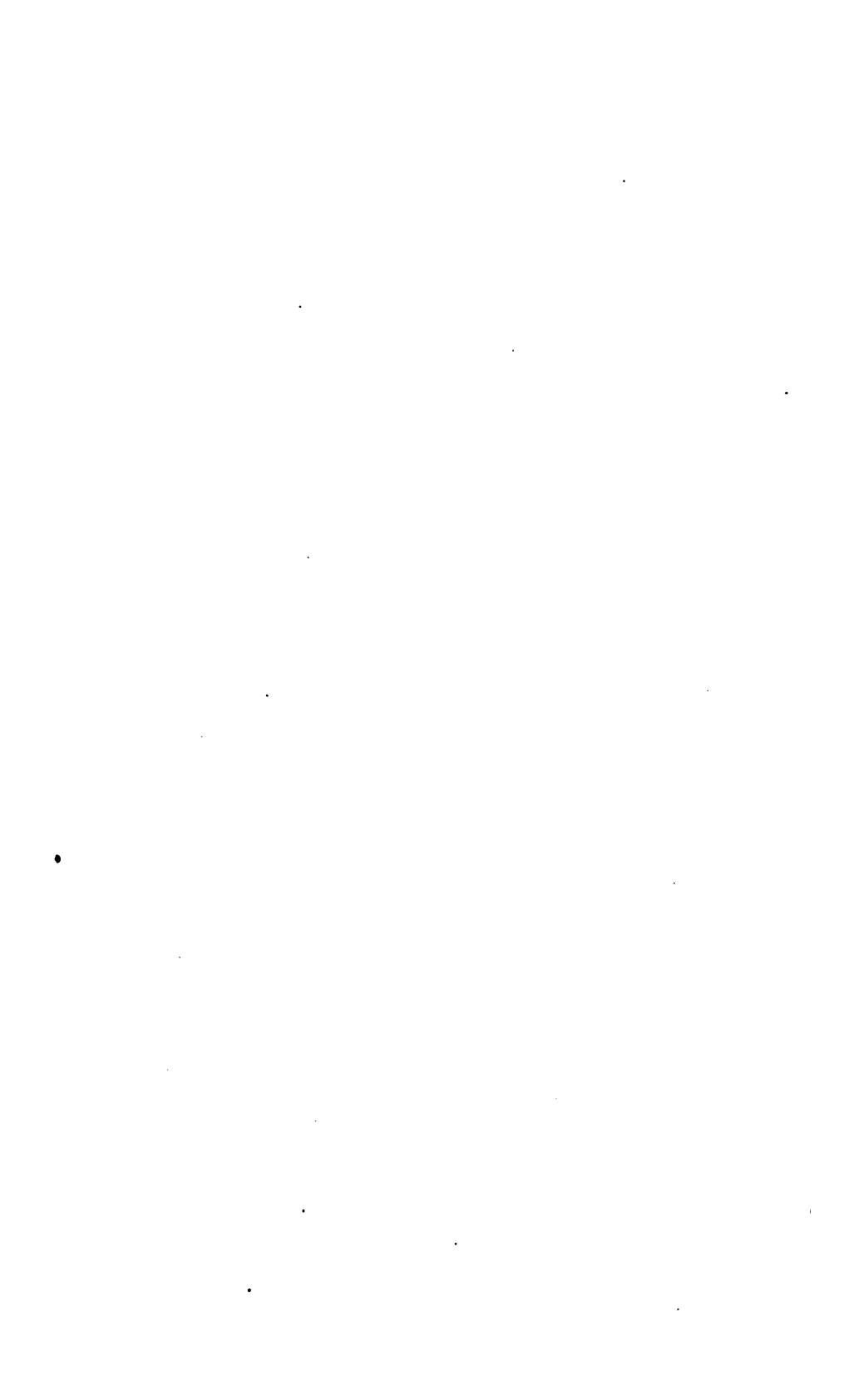


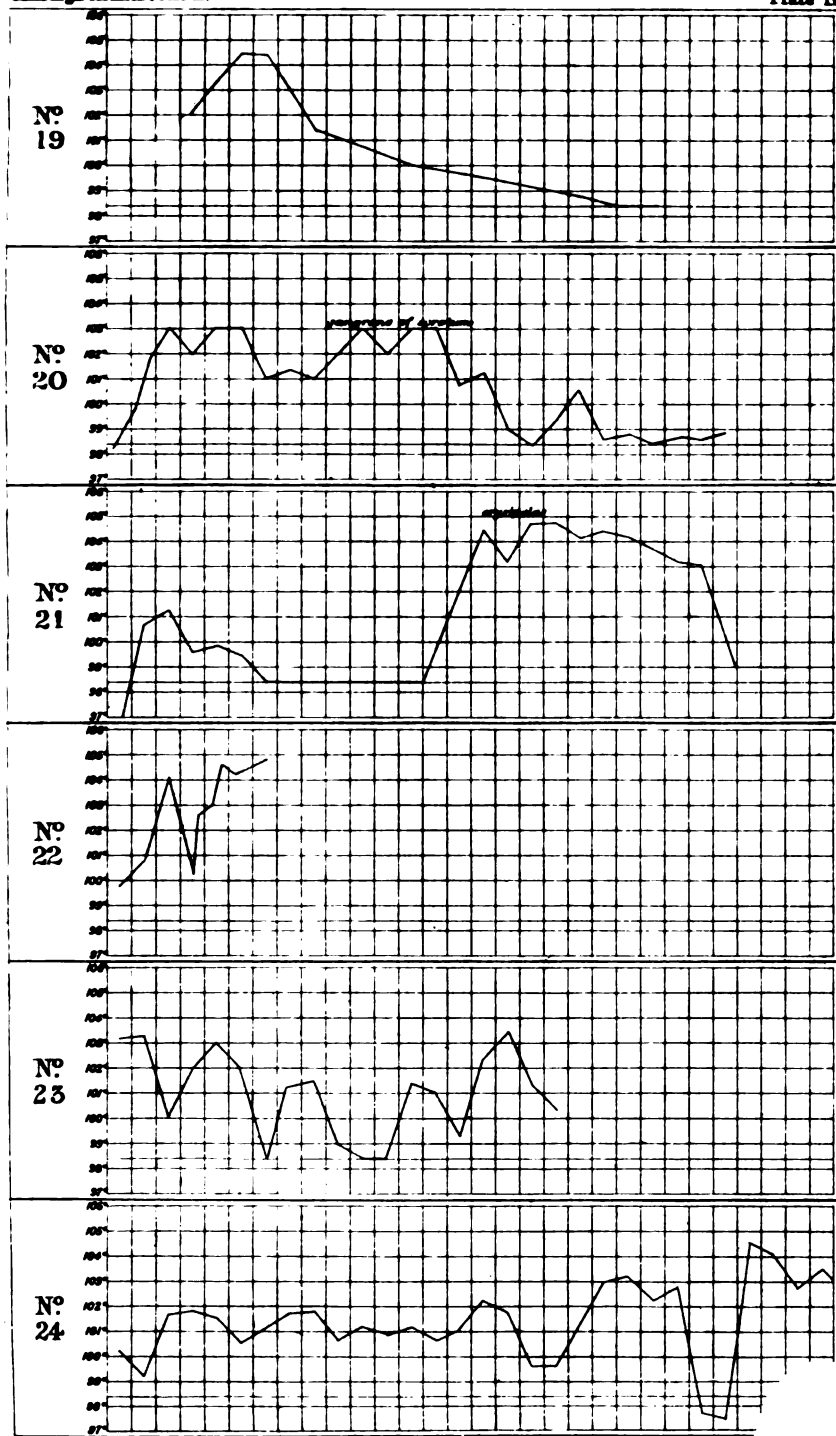
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the breast, and dissected out axilla with great care. She made a very rapid recovery; discharge abundant, but quite sweet; and, except the day of the operation, her temperature did not exceed 99-6°, and her pulse kept under 78. This was a remarkable case, from the extent of surface involved (Fig. 3).

4. *Amputation at Ankle.*—M. G., a very delicate, excitable girl, had her foot amputated for scrofula. Made a rapid recovery. Notwithstanding a very high pulse-rate, only on one day did the temperature pass 100° (Fig. 4).

5. *Compound Comminuted Fracture of Tibia.*—R. S. sustained a compound comminuted fracture of right tibia, with great contusion. It was put up in pasteboard splints, and the wound was simply dressed with wadding soaked in his blood. The wound never required dressing at all, and was not disturbed till he went out. Rapid and complete union took place. The temperature chart tells the history of the case well, as, with the exception of one day, when he was feverish from constipation, it never reached 100°, and on the eighth day became and remained normal (Fig. 5).

6. *Excision of Wrist-Joint.*—J. S., a delicate boy of fifteen, had most extensive strumous disease of left wrist-joint. I excised it, by an incision on the dorsum, and dressed it most carefully antiseptically. Discharge was profuse, but absolutely sweet from the first; and the accompanying is the very remarkable diagram of his temperature. Only once during the first ten days did the temperature reach 99°, and that was the day after the operation, and after a full opiate. He was up on the tenth day, and an outpatient on the thirteenth. The result has been an exceedingly good hand (Fig. 6).

7. *Excision of Knee-Joint.*—Mrs P., æt. 26, had a very bad knee-joint, with abscesses and great destruction of bones. She was losing ground. I excised the knee-joint during a very feverish condition from pain, and the relief was so great that both pulse and temperature fell the very day of the operation. She made a good recovery, and her chart shows a very rapid return to her normal standard, both of temperature and pulse. Discharge in her case was profuse, but antiseptic (Fig. 7).

8. *Excision of Knee-Joint.*—R., æt. 16, was also a case of excision of knee-joint, in which recovery was exceptionally rapid and perfect. I refer to his case only to show how uneventful is the chart of his pulse and temperature. For about five days he suffered a good deal of pain in the leg, after which he could hardly be called a patient at all, as he ate and slept perfectly (Fig. 8).

9. *Lithotomy.*—T., 21, had an oxalate of lime calculus. I performed lithotomy. Rapid recovery. This chart shows the effect of operation and chloroform (?), both in raising the temperature and pulse, and their rapid return to the normal standard (Fig. 9).

10. *Fracture of Skull—Trephining.*—J., æt. 25, healthy and temperate. While crossing a railway with a cart, he was knocked

down by an engine, and sustained a depressed fracture of the right parietal bone, with considerable displacement. He was seen by Dr Kirk of Bathgate, who at once brought him to hospital. I trepanned him six hours after the accident, removing enough to allow me to elevate the depressed fracture, and to remove all fragments and sharp edges. This was done with antiseptic precautions. The wound never putrefied at all, nor had the patient a single bad symptom. The chart shows the uneventful temperature (Fig. 10).

11. *Excision of Elbow-Joint for Disease.*—M.F. He was out of bed on sixth day, and, except on evening of operation, highest temperature registered was 99.2° , and highest pulse 80. His recovery was rapid and complete (Fig. 11).

12. *Fracture of Spine—Recovery.*

The following case is interesting as one of recovery from a very severe lesion of the cord. The chart shows an encouragingly low series of temperatures.

D. H., æt. 21, a strong young painter, fell from the roof to the floor of the Southminster Music Hall in August 1872. On admission, complete paralysis of motion existed below middle of trunk; sensation was little interfered with; bladder was paralyzed. The fifth and sixth dorsal vertebræ were found to be forced into one at an angle, probably both broken.

After very distinct symptoms of early spinal meningitis, lasting for about eight days, he began on the eleventh day to recover power over his bladder, and on the 15th to move his toes; on the 21st, to draw up his leg; and by the beginning of December he had nearly regained full strength of his limbs.

The only high temperatures during the whole case were noted first at the time when he began to be able to move his limbs. His general health at the time was satisfactory; and I am quite unable to account for the altered temperature (Fig. 12).

13. *Fracture of Base of Skull—Scalp Wound.*

S., æt. 22, mason, sustained a very severe scalp wound by a fall of a heavy stone, which killed his companion. He was suffering from symptoms of concussion, bleeding from the upper part of nose, and an intense ecchymosis filling up the orbits and protruding the left eye.

Diagnosis.—Fracture of base of skull anteriorly, involving the sphenoidal fissure. For nearly twenty-four hours he was insensible, and for five or six days confused and delirious. Temperature and pulse kept down, and he made a good recovery. The large scalp wound healed well, except for the occurrence of one abscess where the scalp had been deeply torn off the pericranium. This was heralded by a single high register of temperature two days before the abscess pointed (Fig. 13).

II.—*Cases where Temperature warned of the approach of mischief.*

14. *Inguinal Hernia, and Operation.*—C., æt. 50. Inguinal hernia, strangulated for five days; operated on in usual manner; bowel inflamed; vomiting and local tenderness indicated a local and limited peritonitis, the symptoms of which lasted for four days; well indicated by the temperature, not so well by the pulse; good recovery (Fig. 14).

15. *Housemaid's-knee and Pneumonia.*—M., æt. 16, admitted to Ward I., supposed to have housemaid's-knee. She was very feverish; pulse 110; temperature 103°; and along with a slight bursitis of right knee had a pneumonia of base of right lung. Aconite brought down pulse and temperature. (See diagram.) Abscess was opened when ready, but was putrid; sloughs separated; discharge was copious and foetid, and again temperature rose. She had a long convalescence, but eventually got quite well (Fig. 15).

16. *Necrosis and Smallpox.*—L., a delicate boy, seventeen, had extensive necrosis of right femur. I cut down on it and trepanned it, removing a large exfoliation. A pulse of 144 after operation rapidly came down to 86 on the fifteenth day. Unfortunately, temperature was not recorded. Limb was nearly well and quite sweet, when the temperature in four days went up from 99° to 105°; and an attack of smallpox solved the question, from which he made a good recovery (Fig. 16).

17. *Excision of Mamma and Axillary Glands.*—M. W., fifty-three, had a scirrhus of left breast with glands in axilla. Skin of breast free, but very thin over the tumour. I removed the breast and cleared out axilla in the usual manner. For three days her condition was perfect—no fœtor, no fever; but on fourth, temperature went up, and remained at 102° for three days, during which the lower flaps sloughed. Her recovery was tedious, though eventually perfect; and it is now nearly a year since the operation, and there has been no recurrence of the disease (Fig. 17).

18. *Amputation at Shoulder-Joint for Spreading Gangrene, the result of Accident.*—18th March 1873.—B. K., forty-five, of intemperate habits, dislocated his left elbow by a fall (dislocation was compound) on 11th March. It was reduced and treated by Dr Kirk of Bathgate. Gangrene of arm set in. Patient refused amputation till to-day, when Dr Kirk persuaded him to come to the Infirmary.

State on Admission.—Limb cold and becoming black; covered with petechiæ; sensation gone as far as axilla on inside of arm; redness extends over shoulder and on chest and back; pulse thready and feeble, 108.

Operation.—Assisted by Drs Duncan and Chiene, I amputated at shoulder-joint half an hour after his admission to hospital. The vessels had to be divided very high up, and no covering could be got from the floor of axilla, from spread of the gangrene. Very

little blood was lost, as Dr Chiene compressed the subclavian on first rib. Flaps came together, but the skin looked very doubtful. Case was dressed antiseptically.

The case recovered without a bad symptom. Twice the temperature indicated mischief: once it was explained by the putridity of the discharge, notwithstanding all our care; and the second time by the occurrence of further sloughing in the cellular tissue on the eighth day after operation. The skin fortunately retained its vitality.

Hæmorrhage.—On 6th April, nearly three weeks after the operation, I was sent for on account of hæmorrhage from the nearly cicatrized stump. On taking down the dressing, it was found merely to be oozing from granulations easily stopped by a pad. After this his recovery was uninterrupted, and he left hospital on 8th May (Fig. 18).

Remarks on Case.—1. Recovery under these conditions is rare, as the gangrene is apt to extend to chest and shoulder. 2. The temperature was high, considering that the wound was nearly sweet during the whole course; but the two highest registers are explained by the putridity and sloughing, as noticed. 3. The pulse was uniformly high and weak, but became regular and improved in quality as the case went on. The patient's habits of intemperance may account for this to some extent.

19. *Case of Pyæmia, and Abscess of the Diplœ, after a Scalp Wound—Trephining—Cure.*

16th April 1873.—I was asked by Dr Fergusson, of Leslie, to come at once to see Mr M., farmer.

History.—The patient, a large, powerful man, had, three weeks before my visit, been thrown from his gig. His foot had caught in the reins, and he had been dragged along a hard road for a distance of upwards of fifty yards. He sustained a severe scalp wound near the vertex, triangular in shape, and exposing the bone. His other injuries were comparatively trivial. For some days all went well, and the wound healed rapidly in six days. In eight days before my visit his case had become gradually a very anxious one; he had high fever, intense dyspnœa, recurring rigors, a pulse of about 92, and a temperature varying from 102 to 104·4.

Examination.—Patient's forehead was bedewed with a clammy sweat; his appearance very bad; his respirations *sixty per minute*. Over both lungs crepitant râles were heard, with patches of dulness, not confined either to base or dorsal region. His sputum was rusty, his breath offensive, with the peculiar heavy smell. The scalp wound was almost entirely healed, but one small point showed a little moisture; and Dr F. informed me that two days before he had noticed the scar swollen and œdematous. Probe strikes bare bone.

Diagnosis.—Pyæmia, from inflammation of the diplœ, which has

probably gone on to suppuration, and, possibly, to dura-matral abscess.

Prognosis.—Very bad.

Operation.—Giving the patient chloroform, I made a crucial incision over the seat of injury, large enough to allow me to put on the largest crown of the trephine, with which I removed carefully a circular portion of bone. On reaching the level of the diploë, the outer table came away in the saw, and a quantity of thick, curdy pus oozed out from the centre of the bone, and round the edges of the wound. In case of any matter below the inner table, and seeing it had been split, I removed a circular portion of it also, and we were glad to find the dura mater apparently sound and healthy. Very little blood was lost; and we dressed the wound simply with a little weak carbolic acid lotion. On 28th April, I received the most favourable account of him from Dr Fergusson; and I am glad to say the patient has made an excellent recovery.

For the details in the accompanying chart I am indebted to Dr Fergusson. It shows well how the temperature and respiration go together, and indicate mischief better than either the number or quality of the pulse (Fig. 19).

Note.—The charts shown to the Society in this and nearly all the other cases, indicated the pulse as well as the temperature, and in some the respirations also.

20. *Gangrenous Erysipelas of Scrotum—Recovery.*—T. R., a temperate and otherwise healthy fisherman from Shetland, was admitted into my wards on 5th Dec. 1872, with a large ulcerated scirrhus of right mamma.

Operation.—7th Dec.—I removed it by an ovoid incision, very freely exposing a large part of the pectoral inside, and leaving a very large gap, seven inches by three at broadest part, the bulk of which, however, could be brought together by sutures. It was operated on under spray, and dressed with the strictest antiseptic precautions. For five days the wound looked exceedingly well, kept sweet, and, except for a very faint blush under the dressings, all looked promising. There was a drainage tube in the wound, which ensured against bagging of pus; but from the day of the operation the temperature went up to 102° and 103°, and remained persistently high. This I regarded as very alarming, and was not surprised to find all action cease in the wound, a glazed surface form over the pectoral muscle, and appetite and strength gone.

20th Dec.—To-day, the scrotum became suddenly and enormously swollen to size of adult head, and black in centre to the extent of about a hand's breadth. So at visit on 21st, when I saw it, I made a free incision about nine inches in length right through it, and ordered charcoal poultices. Gangrene was checked, and in a few days sloughs separated, and all the symptoms rapidly improved. Another abscess formed in the forearm, and a third in

the calf, both of which were opened freely, and soon healed, and eventually the patient made a very complete recovery.

Remarks.—Gangrenous erysipelas of scrotum is rare, and recovery is still more rare. This patient had never had any urinary symptom, and there was no stricture nor any other reason why the scrotum should have proved a weak point. The temperature chart is exceedingly instructive, showing how much sooner evidence of the impending mischief was given by the temperature than by the pulse; and, again, how absolutely inoperative the forearm abscess was in raising the temperature, though it was large, because it was kept quite sweet all through its course (Fig. 20).

21. *Gangrene of Scrotum.*—A. F., æt. 44, intemperate, was admitted to my ward on the 25th September 1872. He was labouring under delirium, and his testicles and scrotum were swollen. There was no history of injury, nor any indication of urinary abscess or extravasation.

28th Sept.—Dr John Duncan made an incision in the middle line, evacuating blood and serum.

1st Oct.—Nearly the whole scrotum sloughed away, leaving testicles and part of root of penis quite bare. In about six weeks union was complete, and a good, healthy-looking, but tense scrotum resulted.

I mention this case, in which, unfortunately, the temperature was not taken at the first, merely as an example of a rare malady rarely recovered from.

III.—*High Temperature, with Fatal Issues.*

22. *Excision of Great Toe Joint—Erysipelas—Death.*—F. P., 57. —Disease of tarso-metatarsal joint of great toe; a healthy old body.

4th Jan.—Joint excised; nothing to note. All went well till 18th. Pulse and temperature had been normal; discharge copious but healthy.

18th.—Rigor; temperature, 104.5°; pulse, 126.

19th.—Foot and limb swollen; erysipelas gradually extending up to trunk; no appetite; slow symptoms of erysipelas continued, without any external abscess, but possibly abscesses of liver and lungs, till she died on the 29th. No post-mortem was allowed.

The chart here is almost sufficient history of the case (Fig. 21).

23. *Suicide by Cut Throat.*—This case, a very bad one, was treated in my wards by Dr Chiene, in my absence, and survived only five days. His diagram is instructive enough (Fig. 22).

24. — *Scalp Wound — Delirium — Erysipelas — Necrosis — Meningitis.*—C, F., 36, intemperate, received a very extensive scalp wound, exposing bone, when drunk. Next day had delirium tremens. Wound dressed antiseptically, but took on erysipelas, which



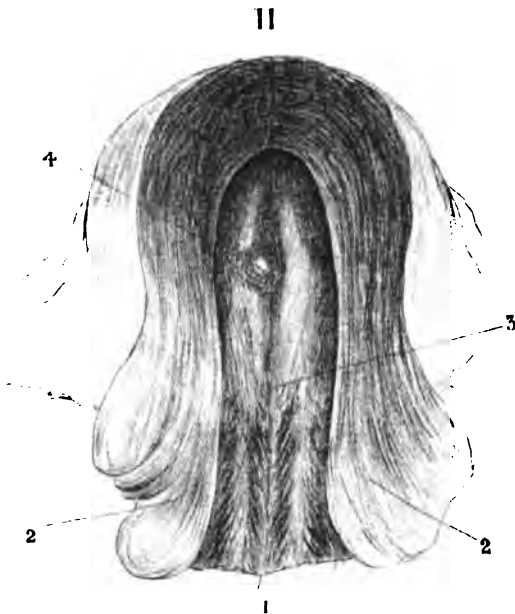
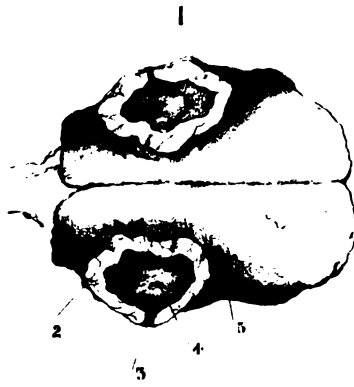


FIG. I.—LEFT OVARY.

- 2. Yellow substance and vessels.
- 3. Orifice through which the ovule escaped.
- 4. Brinous clot.
- 5. Rings of vessels round the corpus luteum.

FIG. II.—UTERUS.

- 1. Os externum.
- 2. Cut surface of anterior wall reflected back.
- 3. Os internum.
- 4. Ovule embedded in decidual matter.

spread; bone exposed and bare; free suppuration; meningitis; death. *Post-mortem*.—Found extensive suppuration, meningitis; no special abscess.

The chart of this case is very descriptive, but is deficient during two days before death. As she was so restless, the temperature could not be relied on (Fig. 23).

25. *Bed-sores—Abscesses—Pyæmia*.—M. S., æt. 19, admitted from Maternity Hospital with enormous bed-sores, necrosis of sacrum and coccyx, with abscesses. For a month youth fought against a hopeless condition. The chart shows a very high temperature, with some curious variations, just before new abscesses (Fig. 24).

ARTICLE VI.—*On the Value of the Corpus Luteum as a Proof of Impregnation; with a Case in which an Unimpregnated Ovum was found in the Virgin Uterus.* By WILLIAM T. BENHAM, M.D., Assistant Medical Officer, City and County Lunatic Asylum, Bristol.

SINCE it has become a rule in the principal lunatic asylums of this country—and in this one amongst many others—to make careful post-mortem examinations in every available case, much light has been thrown on certain pathological conditions which were before clouded in obscurity; and there can be no doubt, that much more may yet be demonstrated by the careful registration of all such appearances as may from time to time be seen by individual pathologists in their especial departments of medical science.

It is not the good fortune of many observers to meet with such a pathological phenomenon—so rare indeed as to be almost, if not quite, unique—as was seen at the examination of the body of a female patient, who died on the third day of the catamenial period, in the status epilepticus, in the Asylum for the City and County of Bristol. This case, I am sure, will be of great interest to many of your readers, throwing light as it certainly does on a medico-legal question of great importance; and I would just premise, that whatever is demonstrable by the facts here recorded, is all the more valuable from the conditions which surrounded the patient at, and for a long time previous to, her death; making it quite impossible that the pathological appearances observed in this case can be explained away by there being the slightest doubt of her having had sexual connexion with any one for at least several years before.¹

Charlotte S., æt. 25, single; of no occupation; was admitted

¹ The plate which accompanies this paper is a copy of the drawings I made of the uterus and ovary whilst in the recent state. They have since been put up in spirit, and added to the pathological collection of this asylum.

into the Bristol Lunatic Asylum in December 1864, suffering from epilepsy. She died on the 26th of March 1873. During the whole of that time she was subject to periodical attacks of epilepsy with excitement, which usually came on a day or two prior to the appearance of the catamenia, which were generally of a normal character, and which ceased at their termination. On the 22d of March, having complained the day before of not feeling very well, she went into the status, and remained in bed in a single room, attended by a nurse, from that time until her death. A mixture of brandy, træ. sumbul., and spt. æth. sulph. (which is usually given here in such cases with good effect), was ordered to be given her every three hours, but she never rallied, and died in the afternoon of the 26th. On the morning of the 23d, it was found that she was menstruating, and was still in that state at the time of her death.

At the autopsy, which was made forty hours after death, the surface of the whole body was found to be much congested, and thickly scattered over the front of the thorax were numerous minute extravasations resembling flea-bites; there was also considerable sugillation of the depending parts; the internal organs, the brain, spinal cord, and their membranes, were all intensely congested; the case thus far presenting the appearances usually found when death has taken place under similar conditions; but the results of the examination of the generative organs were such as to make it incumbent on me to lay them before the profession, and to offer a few remarks on their physiological and anatomical bearing. I can only regret that this most interesting case did not fall into abler hands than mine, so that a more elaborate account might have been given of it, and thus have made it of more value and interest than any attempt of mine can do.

On examining them externally, the organs of generation presented in a marked degree all the highly characteristic signs of virginity. On removing the uterus, ovaries, and Fallopian tubes, they were found to be in a state of intense congestion, the superficial veins standing prominently out, and being filled with dark-coloured blood. The left ovary was more congested than the right, and on its upper anterior surface, situated rather more internally than externally, was seen a bean-like prominence, surrounded by a plexus of bloodvessels all the more apparent from their congested condition; it was of a polished appearance, and of rather a bluish shade of colour. On examining this body it was noticed that its upper surface presented a small nipple-like projection, apparently formed by the distension of a small portion of the peritoneal covering of the ovary, and containing some soft distending material; some small bloodvessels could be observed ramifying round the base of it. At the centre of this prominence was a small irregular dark spot as if a small orifice had existed there, and had been closed up by a clot of blood. On making an incision through its long dia-

meter, there was at once seen an oval-shaped cavity filled by a mass of partially decolorized fibrin, and entirely surrounded—except at the point corresponding to the nipple-like process—with a thick yellow substance of considerable firmness, pierced by a number of small bloodvessels which sprang from the vascular plexus surrounding it externally; and on using a magnifying-glass of low power, these minute vessels could be seen traversing its substance, and losing themselves on its inner edge, which was contiguous to the surface of the partially decolorized fibrinous clot filling up the cavity. On measuring this cavity it was found to be five-eighths of an inch in its long and three-eighths of an inch in its short diameter. The yellow substance surrounding it was found to be of the uniform thickness of one-eighth of an inch, the whole structure measuring seven-eighths of an inch in its long and five-eighths of an inch in its short diameter. On again examining this yellow substance, it was seen to be of a firm granular-like matter, with many minute oil-globules scattered over its surface; and in parts where the bloodvessels were the most numerous they gave it a pinkish-yellow appearance. It will be at once seen that we have here occurring in the virgin ovary a corpus luteum, possessing such decided characteristics as to make it *per se* quite indistinguishable from the so-called *true* corpus luteum of impregnation of the same period, asserted by many writers—Paterson, Lee, Montgomery, Bernand, Deschamps, Müller, Ramsbotham, and others—never to occur but in the ovary of an impregnated female, and consequently regarded by them as infallible proof of a recent pregnancy.

Hoping almost against hope that the ovule, which had evidently been only very recently extruded, might still be found in the cavity of the uterus, a section was made through its anterior wall; and on laying it open, there was seen a small circular body, of a reddish-white colour, almost buried in the pulpy purplish-coloured decidual debris, which thickly covered the whole of the interior surface of the uterus. On carefully removing what I could of this decidual matter from its surface, and examining it with an inch lens, it appeared to be more of a pinky-white colour, and having a fine velvety surface, which, under a higher magnifying power, was seen to consist of multitudes of white filaments. It measured, with the decidual matter surrounding it, and which could not be entirely separated from it, one-twentieth of an inch in diameter, and was situated a little more than seven-twelfths of an inch above the os internum, and slightly to the right side of the median raphe of the uterus. As I have already said, under a higher power its surface was seen to be composed of very short white filaments, which gave it much the appearance, only on a smaller scale, of one of the earliest impregnated ova on record, discovered and described by Velpeau as measuring about five-twelfths of an inch in diameter, and the filaments of which were so far developed as to measure one-

twelfth of an inch in length. This ovum he stated to be fourteen days old.

On making a section through this ovule, with the object of examining it more minutely, I noticed that a very small quantity of albuminous fluid escaped from it, and it lost its globular form directly. A partially collapsed cavity of very minute size was seen to occupy its centre; and although I carefully examined the incised ovule and the fluid which had escaped from it, I could discern nothing more of importance.

It would be interesting to know at what time this ovule was extruded—before, at, or on the day after the time of the commencement of the catamenial flow? Whether it underwent the same external changes in its passage to the uterus as the impregnated ovum? Whether, irrespective of impregnation, an ovule in its descent usually takes on the velvety appearance above described? How long it remains in the uterus before passing away? and whether it passes off in the menstrual fluid entire, or in a state of dissolution? Whether the ovule theory—that at each menstruation an ovule is discharged, as in this case—is capable of demonstration by facts which are as yet undiscovered in the human female? One solitary case like this will not go far to answer any of these queries; but it does prove this much, that an unimpregnated ovule can and has descended into the cavity of the uterus, and remained there for some days without being, as stated by some, immediately washed away with the menstrual fluid, or without, as stated by others, having undergone such rapid dissolution as to have immediately passed away; and, what has been denied by many, that an ovule is ever impregnated in the cavity of the uterus itself, is now made extremely probable. There can, I think, be no doubt that, had this girl had sexual connexion, this ovule might certainly have become a fecundated ovum; unless, indeed, what has never yet been demonstrated is the case, that the menstrual fluid, through which the spermatozoa would have to pass to reach the ovule, possesses the power of destroying those bodies immediately on coming into contact with them. Before passing from this subject, let me state that I think this individual case goes a long way to make it probable that where an ovule is extruded it takes place at the commencement of menstruation more frequently than at any other time. This ovule had evidently been extruded for some time—that is, not less than two days; and that it had not been extruded immediately before death is evident from the fact of its having been found in the cavity of the uterus, embedded in the decidual debris. It must, therefore, have taken some time to have been conveyed there, considering the distance it had to travel; and, besides, the condition of the fibrinous clot filling the cavity it left makes it certain that it had been extruded at least two days previously. It appears, then, that in this case the ovum was extruded at the commencement of menstruation; and that it is usually so in other cases seems, I think, to be indi-

cated by this, that at the time of, and for a day or two previously to, menstruation, a much greater supply of blood flows to the ovaries than at any other period; and the fact of the distended Graafian vesicle sharing in the increased vascularity of the whole structure at that particular time, makes it extremely probable that the extra pressure then put upon the captive ovule should be sufficient to cause it to burst its coverings and to become extruded, and should it not be sufficiently developed to do so at that period, in all probability it would not become extruded until the ovary should be again the subject of increased vascularity; and this seems to show the reason why a menstrual period often passes without the extrusion of an ovule at that particular time. It would appear, then, that the increased vascularity which takes place at the menstrual period goes a long way to explain the mechanism by which an ovule is extruded more frequently at that than at any other time; also there can be no doubt that the increased vascularity is of great importance in producing those changes in the Fallopian tubes and their fimbriated extremities, which result in those parts of the sexual apparatus taking on the turgid and erectile character necessary for the due performance of the function of grasping the extruded ovule and of conveying it to the cavity of the uterus. But where, in cases by no means uncommon, an ovule is extruded and becomes impregnated at an *inter-menstrual* period, I am bound to believe, from instances which have come under my own observation, that it is owing to the fact that, from some exciting cause more or less potent, the ovaries have taken on an increased vascularity of action resembling that incident to menstruation, and sufficient to extrude a well-developed ovum at an abnormal time. Thus, the pleasures and excitement of the opera or the theatre, of the "mazy dance" and supper-rooms, of the merry dinner-party, or of the return home after a prolonged absence, have doubtless often been the cause of that increased rush of blood to the ovaries and Fallopian tubes, which has resulted in the extrusion and possible impregnation of an ovule at an *inter-menstrual* period. In support of this I may just mention a case which has occurred since the foregoing was written, affording considerable negative evidence that such is the case:—E. T., æt. 26, an epileptic, died under precisely similar conditions to the last patient; but although her death happened on the fourth day of menstruation, her uterus, ovaries, and Fallopian tubes presented only very slight, though sufficient evidence of such being the case; there was only a small degree of increased vascularity, and on examination it was quite evident that no ovule had been recently extruded, although the right ovary contained a Graafian vesicle of such mature development, that it was easy to conceive an ordinary amount of ovarian vascularity must have resulted in its extrusion; but the fact that only the very slightest degree of congestion was present would sufficiently seem to indicate the true cause of the non-extrusion of the ovule in this parti-

instance. I before suggested that the non-extrusion of an ovule might be, and probably was, due to the state of the development—or rather of the undevelopment—of the ovule itself; but it would now seem that the *amount of ovarian vascularity* has also much to do with it; for in this case, although there was a fully ripened Graafian vesicle on the surface of the ovary, the amount of congestion was too slight to have any effect sufficient to cause the extrusion of its ovule. I would also add that there were the remains of two former corpora lutea, well marked in character, the one more so than the other; and I think it extremely probable that they were the result of the two previous menstruations, which I ascertained were of a severe character, and accompanied by considerable proof of a well-marked vascular disturbance.

Let me now pass on and state, in as few words as possible, the bearing this case has on the question of the value of the corpus luteum as a test of impregnation.

It has been observed by Dr Dalton, that the corpora lutea following menstruation and impregnation, for some time take the same course of development; and that there is the same rupture of the vesicle, the same extravasation of the blood, followed in either case by the gradual absorption of the colouring matter of the clot, and hypertrophy of the membrane of the vesicle; and thus far he agrees with those who look on the sign of the so-called true corpus luteum as fallacious; but then he goes on to state "that the corpus luteum of impregnation continues to develop itself, and does not become decidedly retrograde until the termination of the pregnancy, whereas the corpus luteum of menstruation reaches its maximum of development at the end of three weeks, and then rapidly becomes atrophied." There is, however, considerable doubt on this point, and it is by no means clear that such is often the case. Bischoff, amongst many other observers, may be quoted as believing that the so-called true corpora lutea may be produced independently of impregnation, and that they consequently cannot be received as proofs of pregnancy. Casper believed that the corpora lutea of menstruation "were in no way distinguishable from those of impregnation;" and Kirkes states that similarity in the character of the two kinds of corpora lutea "necessarily renders unsafe all evidence of previous impregnation founded on the existence of a corpus luteum in the ovary."

That these authorities are right in their conclusion, this case, I think, materially helps to prove. The fact of a corpus luteum of menstruation having been found to so exactly imitate those of impregnation, if, indeed, this was only a *solitary* instance, must considerably weaken the theory still held by many, that a so-called true corpus luteum is a sure sign of impregnation. When we come to consider why impregnation should make such a vast difference in the appearance and structure of the corpus luteum resulting therefrom—as is said to be the case—it is by no means easy to see the reason why such should be so.

If it could be proved that in every case in which impregnation takes place, it does so whilst the ovule is still in its vesicle in the ovary, and at no other time or place whatever—and this, connected with the fact that the corpora lutea in the impregnated female as a rule possess such decided characters—it would be only fair to suppose, although, perhaps, even then difficult to demonstrate with our present knowledge of the subject, that the proximity of the fecundating element which causes such vital changes in the ovule itself, might have some sufficiently potent action on the structures immediately surrounding and in actual connexion with it, to account for the characteristic changes they are known to undergo. But that it is not the case that fecundation takes place in the ovary alone is sufficiently proved by the fact of the unimpregnated ovule having been found, as in this case, elsewhere than in the ovary, at the same time possessing all the highly characteristic features which distinguish it, when it is examined, in the Graafian vesicle itself, and therefore quite as capable of undergoing fecundation when brought into contact with the necessary elements. Granted, then, that the ovule may become fecundated away from the ovary itself, we cannot suppose that the fecundating material in that case could have any direct material action in the formation of the corpus luteum, which would, in that particular case, be found to exist; but under such circumstances the resulting corpus luteum, not having come into contact with the fecundating material, would certainly have the same formation as it would have did fecundation not take place at all. Can we, then, be surprised that, subject to the same early conditions—subject to the same absence of contact with any fecundating material—the corpus luteum of the ovule, which has passed on into the uterus, and has only received impregnation after remaining there for some time, should exactly resemble the corpus luteum of the ovule, which has passed in the same way to the uterus, but which in the end has not become impregnated at all? We must, therefore, conclude, granting the absence of the fecundating element in both cases, that there is no reason why the corpus luteum of menstruation should not exactly resemble the corpus luteum of impregnation in all such cases as come under the conditions I have named. In fact, it does so, as I hope I have sufficiently demonstrated; and, consequently, it follows that the presence of a so-called true corpus luteum, of at all recent formation, in the ovary of a female, has not the slightest legal value whatever in determining the question as to whether impregnation has taken place or not. If that is proved, as I believe it is, by the facts I have brought forward, it should be of considerable importance in its medico-legal as well as in its anatomical and physiological bearings; for, in one case at least recorded by Dr. Guy, the existence of a corpus luteum was held to *prove* that conception had taken place, when the uterus itself presented not the slightest signs of such having been the case.

Much more might be written on the facts I have endeavoured to

bring forward, but so much has at various times been said on the subject, that it has not been my object to go over that which has, from time to time, been written by others; but rather to produce such new facts, valuable in themselves, as my case demonstrates, with the hope that they might, instead of being buried in obscurity, be of service to those who make this and allied subjects their especial study.

ARTICLE VII.—*On Endometritis Decidualis Chronica, as a Cause of Abortion in some Cases of Displacement of the Pregnant Uterus.*
By KRONID SLAVJANSKY, M.D., Lecturer on Midwifery and Diseases of Women at the Imperial Medico-Chirurgical Academy of St Petersburg; Corresponding Member of the Obstetrical Society of Edinburgh, etc.

(Read before the Obstetrical Society of Edinburgh, 9th July.)

THE pathological alterations of the tissues of the uterus and its appendages which give rise to and produce abortion, have been very slightly investigated until now, and therefore are not well known. Many of the symptoms which are present in one series of abortions, and absent in others, cannot be well understood, because our knowledge of the pathological alterations producing them is very insufficient, and therefore these symptoms have not the important diagnostic value which they otherwise ought to possess.

Last year Dr Phillips,¹ in a paper read before the Obstetrical Society of London, narrated a series of cases of retroflexion of the uterus, which had given rise to abortion, and he also remarked the well-known fact of long retention of a portion of the ovum after such abortions. The discussion produced by this paper was interesting, the question raised being—What is the cause, and what pathological alterations take place in order to produce this kind of abortion? Dr Barnes thought that the displacement of the uterus necessarily induced a morbid condition of its tissue, especially engorgement of the body and dilatation of its cavity. Dr Braxton Hicks was of opinion that, in consequence of the dependent position of the uterus, the height of the column of blood was increased to the amount of three inches, the gravitation of which would retard its return into the veins, and thus assist in producing extravasation and the consequent death of the ovum. Drs Rasch and Braxton Hicks have attested the fact of a difficulty in the discharge of the dead ovum in the abortion produced by retroflexion of the uterus. Dr Bantock mentioned one case where he had seen abortion produced by anteversion of the uterus.

Having had the opportunity of investigating the discharged ovum in two cases of abortion from displacement of the uterus, I found some alterations which enable us to understand better the

¹ Transactions of the Obstetrical Society of London, vol. xiv., 1872.

symptoms that are observed in some cases of this kind of abortion. The results of my examination of these two specimens I purpose now to lay before the profession.

CASE 1.—Marpha Leiman, a soldier's wife, 25 years of age, multipara, a native of the province of Tobolsk, coming into the clinical hospital, presented the following *status præsens*. Of the medium size and fair complexion, with the bones and muscular system well developed, the skin loosely adherent to parts beneath, soft, especially on the belly, where it is very flabby, and presents many cicatrices, resulting from having been pregnant; the mucous membrane of the lips and eyes being very pale; pulse very weak and fast, 100 per minute. Nothing abnormal was elicited by investigation of the organs of the chest and abdomen. The lower part of the belly, immediately above the symphysis pubis, is painful on pressure, especially when pressed deep; so that the external examination can give no results.

The internal digital examination shows that the mucous membrane of the vagina is very smooth, the rugæ not well marked, and covered with the ordinary vaginal mucus, slightly tinged with blood. The temperature of the vagina is not increased. The vaginal portion of the cervix is displaced backwards, lying in the excavation of the sacrum, and slightly depressed in the pelvis; it is enlarged in bulk, and very soft; the external os is open, 2.0 cm. wide. Through the os internum the finger cannot be pushed. From the cervix there flows continually a moderate quantity of mucus, slightly coloured with blood. The uterus is enlarged to about the size of a man's fist, movable, anteverted, and anteflexed at the seat of the os internum, to the extent of at least 90 degrees; so that, by the bimanual investigation, its fundus could be easily found lying just above the symphysis pubis. This displacement of the uterus can be corrected with the fingers, but when the fingers are withdrawn, the organ resumes its former position. The tissue of the uterus is slightly indurated and painful on pressure. The posterior vaginal cul-de-sac shows nothing unusual, but its lateral parts are painful when deeply pressed; no resistance or swelling of the uterine appendages can be felt.

The subjective examination shows that the patient complains of extreme weakness, preventing her walking; of giddiness, increasing when she sits or stands; and of slight pains in the lower parts of the belly.

From the history (*anamnesis*) we obtain the following data:—Menstruation regular and painless, beginning in the fifteenth year. Has been three times confined, the pregnancy always normal, the labours natural, and the children born alive. In the time between her various confinements she has always been healthy, and post-partum hæmorrhage has never been observed. Menstruation always began when the suckling was stopped. The last delivery happened

under inconvenient circumstances while travelling in the province of Perm in the end of 1869. The day after her delivery she walked and attended to her usual occupations. During a few weeks after her delivery she observed some bloody discharges; and during the whole time of her travelling, till the regiment arrived at St Petersburg in the month of May 1870, she suffered from a copious white discharge. At the end of that month she noticed all the subjective symptoms of pregnancy just as she had formerly experienced them. Till the month of September the course of the pregnancy seemed quite normal, but in this month, while she suffered from mental depression caused by the death of her daughter, she had a copious discharge of blood from the genital passage, as well as sharp spasmodic pains in the lower part of the belly. These pains, the patient said, were like the pains of labour. From this time she felt a fulness in the lower part of the abdomen, and the presence of some foreign body in her womb. This sensation produced in her mind the belief that the child was dead. Till the January following the patient felt herself quite healthy, with the exception of the sensation she experienced in the lower part of her belly. From the beginning of January till the 4th of February she had some metrorrhagia, with severe spasmodic pains in the lower part of the abdomen. The interval between the attacks lasted only one or two days, during which she felt herself more or less healthy. On one of these days she came to the clinical hospital as an out-patient, and from the out-patients' report we can learn the following details: The uterus enlarged as in the fourth month of pregnancy, antelected about 90 degrees, the cervix pushed backwards and presenting many excoriations on its mucous membrane. On the 7th of February the metrorrhagia returned; the spasmodic pains were very severe, particularly on the 12th, 13th, and 14th of February, and eventually caused expulsion of the unlacerated ovum. After this the hæmorrhage and pains ceased; and three days afterwards the patient came to the clinical hospital manifesting the symptoms above described, and brought with her the aborted parts, asking whether she had been pregnant or not.

The discharged ovum was of a spheroidal shape, measuring in diameter 8.0 cm. The external surface was rough in some places, and in others presented a cribriform appearance, and in many places contained extravasations the size of a hazel-nut. On dissection this ovum appeared to consist of a large hollow space with very thin walls (0.25 cm. thick). These walls were in one place thickened to one centimetre, and this thickness was in extent about half the palm of the hand. The whole space contained a serous transparent fluid, a little coloured with blood, in which some flakes of fibrin were floating. This fluid also contained the foetus, which was 6.5 cm. in length from the crown of the head to the tip of the os coccygis. Its body was covered with a fine membrane, which was very easily removed. This membrane consisted of fibrin, which in some places

is fibrous, and in others finely granular, and which, in some parts, contained red blood-corpuscles. The foetus was quite fresh, and presented no traces of maceration. The skin and muscles under the microscope showed their structure to be normal, without any trace of retrograde metamorphosis. The foetus was suspended from an umbilical cord five centimetres long, which was fastened to the centre of the above-mentioned thickened part of the walls. The umbilical cord was quite free from twistings. The walls of the ovum consisted of two layers easily distinguishable—an internal one, which had all the properties of the amnion, and the external one, which was thicker, and consisted of chorion and decidua. The decidua could be found on the whole surface of the ovum, but its thickness was not everywhere the same. The thickened part of the walls, which by its position corresponded to the placenta, was very hard, fibrous, and contained only a few places where the spongy structure of the normal placenta could be discovered. On the inner surface of the placenta were many centres of extravasation of different sizes, varying from a pea to an almond, and these were prominent in the cavity of the ovum. The decidua was everywhere dense and solid, and very difficult to tear. Its structure was very much altered, and the tissue was very fibrous; the fibres were thick, and between them the decidual cells could be found in very small quantity, and almost all the cells were fattily degenerated. In the tissue altered in this manner, many white cells were embedded, especially in the neighbourhood of some vessels. From the decidua serotina many processes dipped into the structure of the placenta. These processes ramifying produced a network, in the meshes of which the villi of the chorion were situated. The spaces between the villi and the branches of the above-mentioned processes of the altered decidual tissue were occupied in some places by freshly coagulated blood, and in others by blood coagulum which was in the different stages of its retrogressive metamorphoses, and here we can observe at some parts the development of connective tissue from the processes of the decidual tissue. These alterations in the placenta show us that we have to do in this case with the thrombosis sinuum placentæ in quite the same manner as in the case before described by me.¹ The villi of the placenta fetalis are altered almost everywhere; only in the places where the spongy texture of the placenta can be observed, they were more or less well preserved. In the places where the thrombosis is fresh, they show their epithelium fattily degenerated, and their vessels were not easily distinguished, so that in the position of the villi thus altered, we can see only some connective tissue. In the older thrombi the epithelium of the villi cannot be distinguished, and their connective tissue is finely granular, so that its cells cannot be seen. By microscopic examination the epithelial layer of the amnion can be seen very

¹ Zur Lehre von der Erkrankungen der Placenta. Archiv für Gynaekologie, Band v., 1873.

plainly, but the connective tissue layer of the amnion is considerably fibrous, and its cells are fattily degenerated.

Taking into consideration the clinical history of the patient and the results of the examination of the discharged ovum, we find that we have to do in this case with a pregnancy which continued nine months, and the result of which was the delivery of an ovum that, by concluding from its bulk, could not be more developed than in the fourth month of normal pregnancy. The course of the pregnancy was irregular, and, as we have seen, was combined with some diseases, the results of which were irregular hæmorrhages and spasmodic pain in the lower part of the abdomen. Analyzing this case we must answer the following questions :

What was the cause of the metrorrhagia and the spasmodic pains ?

Why were the fœtus and the ovum imperfectly developed ?

Why did not the abortion take place at the beginning of the disease, but was prolonged for such a long time ?

The answers to these questions are given in the history, the investigation of the patient, and the examination of the diseased ovum.

We recall that during pregnancy the uterus was found displaced, and that this displacement was noticed as combined anteversion and ante flexion of the gravid uterus. The uterus was freely movable, indicating the absence of adhesion. The history shows that the patient was always quite healthy, the menstruation always quite regular and painless, that she was three times confined. These facts contradict the diagnosis of congenital flexion. Considering that in the last childbed the patient was compelled to get up very soon and to walk a great deal, and that at this time she had some symptoms (hæmorrhages and white discharges) of an abnormal condition of the genital organs, we must refer the flexion to the category of post-partum flexions. It is known that this kind of flexion is not always followed by sterility, and that the symptoms of the catarrhal inflammation of the mucous membrane of the uterus may not be always observed ; but, on the other hand, we know that sometimes in such cases the catarrh is very severe, and by the cure of the flexion the signs of the catarrh disappear. In our case we have seen that the patient had some symptoms of endometritis catarrhalis for a long time after her last delivery. We know that such catarrhs do not always produce permanent alterations in the mucous membrane, and that they do not indicate that pregnancy may not take place. In such conditions the patient, a married woman, became pregnant for the fourth time ; but the course of the pregnancy could not be expected to be quite normal, for the generative organs were not quite healthy. She had a displaced uterus, with an inflamed condition of the mucous membrane. The uterus increasing, by-and-by increased more and more its own displacement, in the same manner as we see a development of an

inconsiderable flexion of the pregnant uterus in the first few months of pregnancy by normal conditions. In this condition, the irregularities of the blood-circulation must increase much in the same manner as they happen from the displacement of an unimpregnated uterus. This stagnation of the blood, as is known, produces sometimes considerable permanent alterations in the mucous membrane of the uterus; and thus, by the increase of the flexion, are produced alterations in the decidua in the form of chronic inflammations (endometritis decidualis chronica). Such a development of chronic inflammation as the result of the stagnation of the blood, we are accustomed to see in many other organs. I mention only the development of the connective tissue in the liver, and in the mucous membrane of the intestines of patients with emphysema and some diseases of the heart. The alterations of the decidual tissue as the result of chronic inflammation, resulted in the entire destruction of the decidua, so that we have had before us the diffused character of the inflammation (endometritis decidualis chronica diffusa). The decidua serotina was altered in the same manner. The bloodvessels lying in the tissue of the decidua were compressed in proportion to the alteration, and the circulation of the blood became more and more difficult. As one of the results of these difficulties, we regard the slow development of the thrombosis in the sinuses of the placenta. Considering the difficulties of the blood-circulation in a uterus which is anteflexed, where some degree of stagnation exists, it will be clear that when the difficulties of the circulation produced by the chronic inflammation are added, a slight amount of hyperæmia will suffice to produce a rupture of the vessels, and by this a hæmorrhage. Such a hyperæmia took place in our patient in the beginning of the fourth month of the pregnancy, in the same manner as we observe it in women to take place from mental depression (caused in our case by the death of a daughter). By means of the hæmorrhage the blood-circulation was re-established, and, as evidence of the bleeding, there only remained the centres of extravasation in the decidua. These extravasations lying in the decidua, acting as a foreign body, produce irritation of the peripheral ends of the nerves of the uterus, and in this way induce reflex contraction of the muscular fibres of the uterus, and so cause the before-mentioned spasmodic pains. These contractions of the uterus are produced in the same way as we are accustomed to see them in cases of decidua menstrualis, and in cases of abortion in cholera¹ and in typhus patients. In our case, however, the bleeding and the spasmodic pains were observed during three weeks; and although no medical aid was employed, yet abortion did not take place. The explanation of this phenomenon we find in the alterations of the decidua. We have seen that the tissue of the decidua was found to be fibrous, and could not be

¹ Endometritis Decidualis Hæmorrhagica bei Cholera-Kranken. Archiv für Gynækologie, Band iv., 1872.

easily torn. During the act of abortion, as is known, the mucous membrane of the whole cavity of the uterus is always rent in two parts, one of which remains in the cavum uteri, and the other comes out with the ovum and placenta. Nevertheless, although we had considerable contractions of the uterus, this rent did not take place, because the decidua was very hard and firm. As we learn from the history of the patient, such hæmorrhage and spasmodic pains occurred many times, but without being able to produce the abortion till the time when some alterations in the decidua rendered its separation more easy. The extravasations in the decidua mentioned above, and the partial obstruction of some vessels, produced subsequently an insufficient nourishment of the tissues, so that their fatty degeneration took place, in consequence of which the rent could be more easily made. In the beginning of the pregnancy, when the blood circulated freely in the decidua serotina, the development of the fœtus could advance very well. But when the gradual process of the development of the thrombosis sinuum placentæ took place, the development of the fœtus could not advance so regularly. Now, we have had before us a general arrestment of the development of the fœtus—the so-called micro-development. That the fœtus was living until shortly before the abortion, is obvious from the fact that the tissues of its body were very well preserved, and that we could find no trace of maceration. The sensations of the patient after the first hæmorrhage, which made her think that the death of the fœtus had taken place, are not to be much considered, because these sensations in the lower part of the belly may be produced by many diseases of the uterus ; and in our case there were several.

CASE 2.—Anna Ivanova, a labourer's wife, forty-five years of age, a native of Tamboff, was received into the clinical hospital, under the care of my friend Dr Krivsky, with symptoms of metrorrhagia, on the 6th of April 1871. From the ward-sheet of this patient, we learn the following : First menstruation occurred in her eighteenth year ; it was always regular, and lasted for seven days ; has had eight children, of whom six are living at present. In the beginning of the month of December 1870, the patient thought that she had become pregnant. The last menstruation was in the month of October. One day in the beginning of December the patient fell upon her back on the ice. A considerable contusion resulted, so that the patient could neither sit nor lie on her back for some time. On the 6th of December she observed, for the first time, a bloody discharge, which continued till the 1st January. During this time, she had some spasmodic pains in the lower part of the belly. On the 3d of February the hæmorrhages returned, and continued till the 15th of March, accompanied also with severe spasmodic pains. From this time till the beginning of April the patient considered herself quite healthy. From the 2d April till the day when she came to the hospital, she had some severe attacks of

hæmorrhage and spasmodic pains. By the objective examination the following was found: Patient of medium stature, with very pale mucous membranes. The organs of the chest, the liver, spleen, kidneys, and the intestinal tract healthy. Pressure upon the lower part of the belly produced pain. The internal examination shows the vagina flabby, soft, and pale; the vaginal portion of the uterus large and hard. The external os uteri allowed the finger to pass through it, and by gentle pressure the finger could be pushed through the os uteri internum. In the cavum uteri, there could be felt a foreign body, which presented to the finger the feeling of an elastic bag (the ovum). The portio vaginalis could be felt above the symphysis pubis. The fundus of the uterus was lying in the excavation of the os sacrum. The uterus is bent at the neck backwards about 90°. The uterus is enlarged to more than the size of a man's fist, and is painful to the touch. A little blood was observed to flow from the external os uteri.

Without doubt, we have had to do in this case with pregnancy and the beginning of abortion. The questions were as follows: What were the causes of the hæmorrhage, and its irregular periodical returns during the pregnancy? and, What kind of treatment should be applied? Must we favour the progress of the abortion, or try to avert it?

Taking into consideration the flexion of the pregnant uterus and the history of the first-mentioned case, which has shown us that the hæmorrhages and spasmodic pains, with such a cause as in the case before us, might be the results of the chronic inflammation of decidua, I made the diagnosis of endometritis decidualis chronica, combined, perhaps, with some disease of the placenta. Considering the weakness of the patient as the result of the attacks of metrorrhagia which had taken place, and knowing that such hæmorrhages must return again many times in the course of the pregnancy, and that the development of the foetus was considerably arrested, my advice was to favour the course of the abortion. The patient got ergot and warm injections into the vagina.

On the morning of the 9th April, a mass was sent to me, which had been discharged from the genital organs of the patient. The anatomical examination of the discharged body showed that we had before us the placenta, on the margin of which a few flaps of the foetal membranes could be seen. A piece of the umbilical cord, two centimetres long, was attached to the centre of the placenta. The placenta was 8.0 cm. in diameter, and its centre 2.0 cm. thick. It was very hard, and on its cut surface there could be seen only a few parts where the spongy structure was well marked; all the other parts were hard, of a dark-red or dirty-yellow colour. All these parts were interspersed with foetal villi. The decidua serotina was very thick and hard, and contained many centres of extravasation of blood in different stages of retrogressive metamorphosis. The microscope showed the tissue of

the decidua serotina to consist of a considerable quantity of fibrous tissue with very thick fibres; and between them could be found a few decidual fusiform cells, containing sometimes many nuclei. In the neighbourhood of the centres of the extravasations we could find some traces of fatty degeneration, while the other parts of the tissue were quite free from it. The foetal villi in the few spongy parts of the placenta were well preserved, and their structure was easily distinguished. Their epithelium was everywhere finely granular, and only sometimes could we see their nuclei. The hardening of the placenta was produced by thrombosis sinuum placentæ. The coagulated blood was in different stages of its retrogressive metamorphosis. The foetal villi lying in these places were in different stages of fatty degeneration. The tissues of the chorion and amnion were without any alteration, but showed a slight degree of infiltration with white cells. The foetus and the decidua vera were not sent to me, so that their condition was not observed. From this examination we can draw the following conclusions:—The decidua serotina was altered in the same manner as we are accustomed to find it from chronic inflammation (endometritis decidualis placentalis chronica). The placenta is in the state which is called induration, and is produced by coagulation of the blood in the placental sinuses.

Considering the alterations of the decidual tissue in the placenta materna (decidua serotina), and the symptoms produced by them, we see that this case has a complete analogy to the first case. In both cases, the first impulse to produce hæmorrhage could be easily found. In the first, it was a hyperæmia, the result of a mental affection; and in the second it had a traumatic origin. In the first case, the time elapsing before the expulsion of the diseased ovum was longer than in the second, its course having been left uninterfered with, no medical treatment having been used. In the second, as we have seen, the disease was diagnosed, and the abortion was earlier brought about by suitable treatment.

Both of the described cases are uncommon, considering the great degree of alteration which had taken place. Every flexion does not produce such alterations; and when they do take place, they are very rarely of such a high degree. Analogous circumstances we find in the unimpregnated uterus.

To speak about the cases of slight degree of alteration produced by displacement of the uterus is not my purpose, because I have not had opportunity to investigate the decidua in such cases. I wish only to indicate the probability of such alterations, and especially in the decidua serotina, being the cause of the abortion, as well as of its prolonged course, just as in the above-described cases, but in a less degree.

ARTICLE VIII.—*On Re-Vaccination as a Means of Preventing and "Stamping-out" Smallpox.* By Dr G. HAMILTON, Falkirk.

THE fond and pretty confident anticipations indulged in, both by many of the public and of our profession, that Jenner's great discovery would, in a few generations, almost make smallpox a thing of the past which had disappeared from among us, have received a rather rude shock during the last few years. It has become clear either that our circumstances have altered, or that we were miscalculating our dangers; for in a large portion of the empire the explanation given as to the spread of former epidemics will not now hold good. Twenty-three years since, when the Stirlingshire Midland Railway was being made, I saw a pretty severe variolous epidemic, which had its focus among the workmen employed in making that railway. A large majority of them were Skye men, who had not been vaccinated, and who were determined that they and theirs should remain so; and, as we had then no law to compel vaccination, the disease spread with great rapidity, infecting not only the Highlanders and their families, but also the inhabitants of the villages where they resided. Since then, the position of matters here has been completely altered: the Highlanders have gone, and our own population have willingly submitted to a most excellent law, making vaccination compulsory. As one of the vaccinators for this parish, I can testify to this being so thoroughly the case that vaccination here may now be said to be universal. And yet, notwithstanding such a very satisfactory condition in this respect, we have for the last eighteen months had a pretty smart, and in some few quarters deadly, variolous epidemic prevailing in different parts of this and the adjoining districts.

This, it must be confessed, is not a very pleasing state to be placed in; and here, as elsewhere, it has given rise to great and anxious concern, both to families and to the authorities, and certainly ought seriously and especially to engage the attention of the medical profession. In giving expression to a few passing thoughts on this subject, I would remark that it may be hoped that this, like other epidemics, may be temporary in its duration, and that its exciting cause or causes will pass away, and leave us in our former comparative immunity from its attacks. But how little do we know with exactness of the laws which guide the diffusion of these zymotic diseases! We have been able to see, in certain well-marked and striking instances, such as Asiatic cholera or cattle plague, that the mysterious and unwelcome visitor has advanced even at an ascertained rate per diem, and in a definite direction, towards our shores; that it has then, like shower clouds, become diffused over our land, pouring down its baneful influence, as it were in torrents, on certain spots, while on others it either falls not at all, or only like a passing shower; and that after a time again

the deadly plague, whatever may be its nature, disappears, and ceases during long intervals to molest us. Though less known, because less deadly and appalling in their effects, similar laws, could we trace their operation, quite possibly direct the courses of other affections, such as influenza, measles, scarlatina, and smallpox; and latterly we can hardly account for the general spread of variola among us, without assuming the existence of some special predisposing cause of a widely-diffused kind.¹ Whether this cause may be temporary, or will remain permanently with us, it would seem that our efforts in the meantime, if we wish to benefit the community, must still mainly be directed to preventing or "stamping-out" this disease; for, from all I have seen or heard, its treatment has undergone but very little improvement in our day.

Various means towards these ends have, as a matter of course, presented themselves to the minds of almost all who have been engaged practically in combating the disease, and have been used with more or less advantage according as circumstances have allowed of their being efficiently worked. In large towns, hospitals must have been of vast advantage in this respect; and, in all cases, establishing a kind of domestic quarantine, and disinfection, must also, no doubt, prove efficacious. Unfortunately, however, in a very large amount of cases among our working population, where small and crowded dwellings most favour the diffusion of the virus, disinfection can be but partially applied, and neither separation nor removal is available; and hence the enormous im-

¹ An abstract of a Report on Smallpox in Milan is given in the British and Foreign Medico-Chirurgical Review for January 1873, from which the following is taken:—"The reporters, after a short historical survey of a general kind, tabulate the most extensive epidemics of the disease occurring in Milan from 1752 to 1789, from 1830 to 1843, and from 1852 to the present time. From their tables we gather that, between 1752 and 1789, there were from 1000 to 2000 cases of the disease in every year; from 1830 to 1843, the average number of cases was from 300 to 400; from 1852 to 1863, in Milan and surrounding district of two miles, the average was about 500 cases per year; from 1867 to 1869, the average was about 300 in the city proper; and from 1870 to June 1872, the proportion for 1870 was 1287; for 1871, 4467; and for the six months of 1872, 491. The figures show that the smallpox epidemic of 1871 was more severe than the worst epidemic of the last century of which there is record—viz., that of 1752—during which there are estimated to have occurred 4068 cases." From the Registrar-General's returns for Scotland for 1872, we learn that in that year smallpox in Edinburgh caused 702 deaths, or 12·88 per cent. of the whole mortality; while in Leith it caused no less than 16·66 per cent. With these figures before us, it is not difficult to imagine the enormous number of cases that must have occurred there alone. Notwithstanding our compulsory and generally successful vaccination, the Registrar-General states that the smallpox epidemic of the last year was the most severe which has occurred in Scotland since the general adoption of vaccination in 1799. Speaking of the eight principal towns, too, he remarks that this epidemic appeared simultaneously in Edinburgh, Dundee, Aberdeen, and Leith, all on the east coast; and that it rapidly declined as the season advanced, so that by the end of August it was almost extinct. In Sunderland, in 1871, smallpox caused 796 deaths in ten months.

portance of finding some other means to stop the ravages of the disease. From all the experience I have had during forty years of practice, re-vaccination has seemed to me the most powerful for this purpose at our disposal. Some surgeons have thought that the original vaccination must in some way have been carelessly or defectively performed to account for recent outbreaks; but in this opinion I do not share, for I have seen the disease spread with rapidity in houses, and clusters of houses, where most of the inmates had been carefully vaccinated by myself, and where the cicatrices (almost always two) on the arms were perfectly well marked.

In practising re-vaccination, it has been only within the last year or two that I have tried to carry it out systematically among all my patients. No doubt, in my own family, and among better-class patients, this had previously been done pretty regularly, especially when any alarm has occurred; and in my own individual case, as I think every surgeon should do, I have repeated the re-vaccination several times;¹ but among the mass of the working population the same has by no means been the case. Very few, indeed, in this district had, until recently, been so. Various causes operated, and still operate, in producing this effect. There is, first, the apathy which exists among the comparatively uncultivated, and the disinclination to put themselves to any trouble or expense where a danger seems distant or slight; and the latter consideration as to expense is in most cases paramount, even when the danger is nearer and deadly. Single individuals may take the alarm, and get re-vaccinated; but I have never yet, that I can recollect of, known a common working man so thoughtful and prudent as to have this done to all his family, *if he had to pay for it*, even though deaths from smallpox may have been occurring all round about him. In a family of six it would cost him, let us say, nine shillings to have the operations performed; and with all the respect which I have for both the prudence and good sense of the Scotch working men, I must at the same time say I know very few who would incur this expense to lessen what they consider the risk of contagion. They must just, they would say, take their chances like other folk.

¹ In the Milan Report, previously referred to, the observation is made that "practice and observation have established that the immunity to small-pox created by vaccination performed during infancy is limited in protective influence by years. The limitation is ten years; and, therefore, between ten and fifteen years of age re-vaccination should be carried out. At a period of more advanced life it should be again repeated." And again:—"Speaking of other (than military) classes of the community, they observe that, while the disease was most prevalent among the lower and in the middle classes, amongst those who are brought most into communication with the populace, one eloquent fact stands forth, viz., that amongst the medical fraternity, including those brought most of all into contact with the disease, but at the same time most safely protected by vaccination, not a single serious example of the affection appeared during the whole period of thirty months."

But, secondly, not only has this apathy existed when a fee has been exacted for re-vaccination; it has also occurred among certain classes of the population when this has been offered to them gratuitously. Among my own "club" patients I have never asked a fee for re-vaccination, and yet I have had the greatest difficulty, until lately, in *persuading* them to allow me to do so. In August of last year, smallpox of rather a deadly type prevailed in a portion of the parish of Larbert, for which I act as parochial surgeon, and the inspector of poor consulted me as to what means could be employed for stopping it. Above all things I recommended re-vaccination. Despairing of getting the people themselves to pay for it, and consequently thinking it useless merely to recommend it to them, he consulted with the parish committee, who authorized me to re-vaccinate without charge all who were willing to accept of it. Accordingly, I issued notices in five different localities that I would attend, on certain days, to re-vaccinate all who might wish it, with the following results:—In three of these, in one of which the officiating clergyman kindly made the announcement from the pulpit, the attendance was *nil*. The population here was mostly composed of miners, and the result, I may mention, was the same as took place among the Skye Highlanders I have already referred to. In another large colliery, which is under my own care, the manager made a similar announcement to the men and their families, and the result was the same. In the other two localities of the Larbert parish, I thought at first that I was to be equally unsuccessful, but by some coaxing, and by sending messengers to the different houses, I succeeded in re-vaccinating in these respectively 47 and 51.

Thirdly, I found, among certain portions of the population, when the disease broke out in their neighbourhood or in their families, not merely apathy, but the most violent prejudices against the performance of the operation. They had a theory that when the two affections met the effect was deadly; and cases were cited to me as having occurred in a district, not far distant, where the patients had died in consequence. I was not able to satisfy myself as to the nature of the cases referred to, but I conjecture they may have been bad cases of modified variola, in which re-vaccination had been tried by some of the surgeons resident there after the eruption had appeared, and in which death had taken place. In their ignorance, the re-vaccination was very likely viewed by friends and neighbours as the cause, or at least a possible cause, of the fatal result; and my patients firmly resisted all my endeavours to persuade them to the contrary, until I had opportunities for demonstrating to them, from many examples, that their prejudice against re-vaccination, even in proximity to smallpox, was unfounded. Every practical person must know how difficult it is to deal with uneducated people in such circumstances. I was, therefore, much gratified lately, when collecting a few materials for this paper, to

have furnished to me by one of the mothers whose prejudices on this subject were formerly among the strongest, the following account of what subsequently took place in her own house and family:—

1st, Mary B., æt. 14, took smallpox first. Had just previously been re-vaccinated, and the scabs were forming on the arms when the variolous eruption appeared. Had on her body about two dozen pocks.

2d, Ellen B., æt. 7, also re-vaccinated, was next seized, and had about a dozen pocks.

3d, Agnes B., æt. 17, vaccinated, but not re-vaccinated. Had one of the most severe attacks of modified smallpox I have ever seen recovered from.

4th, Walter B., æt. 5, also vaccinated, but not re-vaccinated. Very severe also.

5th, Isabella B., æt. 4, re-vaccinated, had on her body four or five pocks.

This woman added that she had repeated to many, in and out of the profession, what she had herself seen, and I have no doubt that it has been the knowledge among my patients of many such instances occurring which has now made it easy for me to re-vaccinate whenever I wish to have it done. In many other instances the effects were so striking as to aid me much. Here is one. In February 1872, a girl in service in Edinburgh took smallpox, and came to her home in this neighbourhood. The case was a severe one, and the house into which she came was small, low-roofed, and crowded with other inmates. At first I thought of sending the patient back to Edinburgh, to the smallpox hospital, but this we found to be impossible. I therefore got her separated as far as possible from the rest of the family, and immediately re-vaccinated all the other inmates that would submit to it. During the course of the girl's illness the mother gave birth to a child, which I vaccinated when ten hours old, and the following occurred:—The infant's vaccination did well, and on the fourteenth day, when the scab had formed on the arm, a papular eruption came out on the face and body, but in a few days aborted and died away, without almost sickening the child. Of the other members of the family re-vaccinated, four sickened and were slightly unwell, and on two of these a few pocks were observed. On the other two none were noticed, though, if examined more narrowly, probably a few aborted pustules might have been found.

Latterly, I have been even more successful, in a good many instances, where one in a large family has been seized. Very slight separation, and immediate re-vaccination of every member of the household, have either prevented the disease spreading altogether, or reduced it to a minimum, by effecting complete abortion of it. Of the latter class of cases I shall immediately have to give some interesting examples.

The experience that I have now had in re-vaccination has been pretty considerable, and the negative evidence which has presented itself has been as yet entirely satisfactory, for I have seen no such case in which death has occurred, or indeed where the attack, when such did take place, has been more severe than those already mentioned. I may notice, also, that though the pustule in re-vaccination has been often incomplete, almost indeed an abortion, it yet has appeared to me to possess an active protective and aborting influence, or at least that in such cases, where there has been infection, it has been of the mildest possible kind.

With the kind of experience of which the foregoing may convey some idea, and having great faith in re-vaccination as a *protective* power, I read with much interest, in the *Scotsman* newspaper of last year, letters from Mr Furley, in which he stated that he had been able, by using re-vaccination, even after the eruption had appeared for a day or two, to abort or greatly to modify the subsequent course of the disease, and I immediately made some trials of this practice, but I cannot say that these were satisfactory. In severe cases I did not find under such circumstances the disease to be appreciably checked, and I therefore gave up practising it. Mr Furley's further experiments in re-vaccination, and the discussion connected with these, which I read in this Journal for December last, again directed my attention to the same subject; and though I saw from this and other communications in the journals, that other practitioners had not been equally successful in repeating his experiments and using his mode of re-vaccination, the balance of opinion indeed inclining strongly against it, it yet seemed to me that, even admitting that re-vaccination might fail to abort the disease, when applied say on the first, second, or third day of the eruption, it does not therefore follow that an anterior application of the same remedy would not be successful in cutting the affection short. This point is adverted to, though I think not sufficiently dwelt upon, in the discussion in the Obstetrical Society. It has to be recollected as a matter of course on this point, that, for practical purposes, the disease commences *when the patient is taken ill*, and that this is generally three, four, or more days before the eruption appears; and it has therefore been to this portion of the course usually pursued by the disease that I have particularly directed my attempts at "aborting," regarding which I beg to submit to the profession a few of the facts I have collected. Although in hospital practice cases are rarely seen in this stage, they are very generally so, if at all severe, by the general practitioner.

I have found two difficulties to stand in the way of attempts to thus abort or "stamp out" the disease,—1st, In the diagnosis for a time being obscure; and, 2d, From a supposed danger in applying remedies which might prevent the eruption coming freely out. As to the first of these, no doubt there is a difficulty, especially when there also prevails, as has been the case in this locality during the

last six months, an influenza epidemic having nearly the same general symptoms. The peculiar full, vibrating, variolous pulse, however, is in most cases pretty distinctive (and is especially useful in diagnosis where smallpox prevails); and, besides, I have not lately found the re-vaccination objected to, even where there may have been some doubt as to what was coming. In regard to the second difficulty, it must be admitted that a very marked and striking amelioration of the general symptoms, and in the beat of the pulse, certainly do take place just when the eruption comes out; but I shall have to show that re-vaccination in the premonitory stage does not seem to prevent, but rather assists, these. I have thought it well, therefore, to err on the safe side, and, wherever there has been any suspicion of impending smallpox, to re-vaccinate. A great many of these have been negative in their results,—that is, the patient has recovered quickly, and no eruption has appeared. I think it would be rash, however, to conclude that in all such instances the cases had been those of influenza, or of some other febrile affection, that had been under treatment; for I confess I have a strong suspicion, that if got at the *very commencement* of the premonitory fever, the eruption may be prevented making its appearance altogether; satisfactory proof on such a point, however, must naturally remain a very difficult matter.¹

Of the *positive results* obtained, the following may be taken as examples which have occurred during my experience:—

1. J. R., æt. 18, living in a house where a patient was in bed with smallpox, was re-vaccinated on 11th March; continued at his work as a miner up to 26th March, when I saw him still going about; says that he has felt a little squeamish at times, and still does so; noticed on his forehead one apparently aborted pustule, which, from its appearance on 30th March, looked to be about the

¹ Here is a case in which it can hardly be doubted the disease never got beyond the premonitory stage. On 3d May, I found W. K. (father of J. K., No. 12, whose case of severe confluent smallpox is subsequently referred to) in bed, with full vibrating pulse, and other symptoms which made me think he had the premonitory fever. This man states that he was vaccinated when young, and the marks are on his arm, and that he had, when about sixteen years old, what he thinks was a mild attack of modified smallpox; notwithstanding this, he had himself re-vaccinated some months since. On 21st April, I again re-vaccinated him, and as it did not show much, I re-re-re-vaccinated him. I took these precautions as he had to be the principal attendant on his son, who was separated from the rest of the family. To-day, 3d May, the symptoms so much resemble those of the premonitory fever that I again put four vaccine marks on his arm. No eruption ever came out, but he continued weak and feverish for ten to twelve days; and on 8th May, a son who slept with him, and who also had been re-vaccinated, had a dozen or two pocks on his body, the attack being of the mildest possible character.

Considerable pains were taken to cut off the communication between the son above referred to and his brother who first took smallpox, and it therefore seems probable that the contagion was conveyed from father to son, who slept together. If this were the case, it would show that smallpox is contagious, even though no eruption may appear—a point regarding which I have not been able to satisfy myself in any other instance.

sixth or seventh day from its first appearance ; and noticed also upon one arm on both days several small spots, apparently abortions. The marks of re-vaccination, which were slightly inflamed for a few days, had small scabs on them on 26th March.

Remarks.—This lad was re-vaccinated on the 11th. If we say that the 30th was the eighth day of the eruption, and add four days for the premonitory fever, he would be re-vaccinated five or six days before the disease showed itself actively ; and here the abortion was all but complete. Indeed, except for the one pock on the forehead, the eruption would almost certainly never have been noticed.

2. Saw B. R. in another locality on 22d February, sick and in bed. His skin was hot, and his pulse quick and vibrating, which immediately made me suspect impending smallpox, in consequence of which I re-vaccinated him. His mother stated to me that she noticed the eruption on the 24th, and I saw him on the 25th, when it was to be seen on the forehead and forearms, but small and sparsely scattered. Feels now comparatively easy. On 27th one pock is to be seen on forehead, and a few on breast and arms, but aborting. On 1st March the papulæ have nearly disappeared. On 4th March he had a return of an old chest affection, which retarded his recovery for a few weeks.

3. D. W., æt. 12, felt unwell on the evening of 19th January. Saw, and re-vaccinated him on 21st January, when no eruption had appeared. Mother states that on the 22d she saw a papular eruption on the forehead. On the 23d I saw this plainly, apparently aborted smallpox. The vaccine incisions are inflamed, and the boy easy and sitting up.

4. W. P., nearly the same as the above, though more severe, the eruption making its appearance the day after re-vaccination, and the patient being well in about a week.

Remarks.—In 2, 3, 4, the re-vaccination showed only a moderate amount of irritation. In No. 2 it was least, though quite perceptible. In the two others a small scab was formed, on from the 6th to the 8th day.

5. On 2d April saw Mrs G., æt. 50, who has been complaining for some days. Face flushed, eyes injected, pulse quick and vibrating. Looks prostrated, and altogether very ill, and impresses me with the feeling that smallpox is about to appear. Re-vaccinated, and ordered warm drinks. 4th April.—Somewhat easier. Papular eruption shining through the skin of forehead especially. 7th April.—Pulse still quick, but feels much better. Two aborted pustules seen and can be felt on forehead, and a few on arms. 9th April.—Pulse still quick, but is much improved, and is able to be up for a little while. Papules going off.

Remarks.—This appears to me likely to have been a very severe case of smallpox, had the re-vaccination not been applied in time,

and, as it was, the patient took eight to ten days more to recover her strength.

6. C. B., æt. 4. Called on 2d April. Feverish, pulse vibrating, and face flushed. Suspected smallpox, and re-vaccinated. On 3d, fever less, but no eruption. On 5th, going about, and one pock noticed on forehead and three on each arm. One re-vaccination has taken.

7. A. D., æt. 27, was taken ill on 2d April, and was in bed when I visited him, within a foot or two of his child, who was ill with smallpox. Pulse vibrating and quick, and altogether seems in premonitory stage of the same disease. Re-vaccinated. On 5th April he was sitting up, and I observed one aborting pock on his forehead, and three on each arm.

Remarks.—Nos. 6 and 7 were mild cases, and the aborted eruptions might easily have been overlooked. Indeed, both wife and husband in the two houses at first declared to me that no eruption had appeared.

8. The following is a group of very striking cases, that occurred in a confined house, where one child lay ill of smallpox:—Mary M., æt. 2 years, in bed and feverish, on 11th March, when she was re-vaccinated. Up next day, and no eruption was observed. James M., æt. 4 years, was re-vaccinated on 11th March. On 26th, was much flushed and fevered, and has been so for two or three days. No eruption. 28th.—Eruption came out yesterday, but is hardly to be seen to-day. Up and free from fever. 30th.—A few aborted pocks are to be seen on face and arms. Alison M., æt. 13, was re-vaccinated on 11th March, but, as it did not seem to take well, was again re-vaccinated on the 21st. On the 26th, pulse is quick, and feels a little sick, but is going about. Both re-vaccinations now show some redness. Up to 30th March no eruption was noticed. Ellen M., æt. 16, began complaining on 20th March, and I saw her on the 21st, with what I considered the premonitory fever, and re-vaccinated her. The fever continued on 22d and 23d, but no eruption showed itself. On 24th, the fever less, and a very few small spots are to be seen on face and forehead, evidently abortions. 26th.—Eruption quite aborted, but still in bed, and a little feverish. 28th.—Still out and in bed, and slightly feverish. 30th.—Up, and pulse quiet. Martha M., æt. 11, was re-vaccinated on 11th March. On 29th was in bed, and flushed and feverish. As first re-vaccination had left little mark, again to-day re-vaccinated. On 4th April found her going about, and on examining her arms found an aborted pock on each arm.

Remarks.—This group is very striking, and produced a great effect on the minds of both father and mother, the latter of whom made great objections at first to the re-vaccinations. The abortion in the last was probably so nearly complete from the girl having

been twice re-vaccinated; and this may furnish a valuable hint, even where there has been re-vaccination, as the premonitory stage again affords us an opportunity of checking the disease. In such cases, I now generally re-vaccinate in four places. Her case could easily have been passed by as one in which no eruption had appeared; and probably her two sisters, in whom no eruption was observed, would have been found like her if they had been more narrowly examined. But for the re-vaccination, the case of Ellen M. would, I think, have been a severe one.

9. On 13th and 14th May, I saw three cases which struck me forcibly. All three had fever, which might be premonitory; but in one of them, I thought the pulse rather softer than I usually find it in smallpox, and I therefore did not re-vaccinate; and this unfortunately proved to be a severe confluent case. The other two I re-vaccinated, each in four places. In two days the aborting pocks showed themselves in both, and the cases terminated most satisfactorily.¹

10. Mary A., æt. 6, on 20th March was lying in bed feverish, apparently with premonitory symptoms of smallpox, as another very severe case occupied the same small room. Re-vaccinated. 21st.—Abortig papular eruption on face and arms. The same continued up to the 24th, the eruption being nearly stationary, the pulse quick, and the skin slightly hot, but still the symptoms mild. 26th.—In the last two days the eruption has taken a leap, and the pustules, though small and scattered, look about the sixth day. They are almost all on the face. 30th.—Face now cleaning quickly from scabs. Going about.

Remarks.—This girl was very likely re-vaccinated very shortly, possibly only a few hours, before the eruption appeared, and the abortive effect, I think, was probably less in consequence of this

¹ One of these was so remarkable as to be worthy of being mentioned more in detail. The following are jottings of it from my notes:—14th May.—J. S., a strong lad, æt. eighteen, has smart premonitory fever. Re-vaccinated in four places. 16th May.—All the scratches seem likely to do. Up, and feels quite easy. A few aborting pocks on face, forehead, and arms. 23d May.—Going about, but, as his father remarked, has got a shake. Abortig pocks well seen on forehead and face. 26th May.—Two of the vaccine vesicles large, and somewhat past the height. Is not so well to-day, and is in bed, and complains much of his head. Pulse quick, but not vibrating, and skin hot. On examining his body there are seen—1st, The aborted pocks, forming little scabs; and, 2d, A new eruption, having in some places, especially on the thighs, a look as if clusters of confluent pocks were making their appearance; in others, circumscribed red patches about the size of marbles, slightly raised above the skin; and in others still, the same red appearance, larger and more diffused. The parts about the wrists are slightly swollen. 28th May.—Much easier, though still in bed, and afraid to come out of it from fear of fainting. Pulse 88, soft. The second eruption to-day is not quite so red as on 26th. In some places it is more like nettle-rash than anything I have ever seen, but is more prominent. The large patches and clusters are still to be seen. 30th May.—Well, but very weak. Pulse quite natural. Both eruptions gone, the only thing left being some small scabs from the aborted pocks. *Remarks.*—I do not recollect ever before seeing anything like this in my practice.

proximity. I may give the two following cases as examples of re-vaccination immediately *after* the eruption had appeared.

11. Saw Mrs H., æt. 25, about noon on 20th May, in bed, and feverish. She and the neighbours had this morning noticed an eruption on face and arms, which they were afraid might be smallpox, which was prevailing near her residence. No doubt it is, but apparently of a mild character. Re-vaccinated in four places. Saw her again on 22d and 24th, when she was still in bed, and complained of being very weak, but the eruption has aborted. Pointed out to me particularly three aborted pocks on arms and two on forehead. On 27th May the two re-vaccinations and the pocks formed small aborted scabs. Up, though still weak.

12. J. K., æt. 22, complained on 17th April, and I saw him on the afternoon of the 19th. The eruption was stated to me not to have appeared on that morning, but I saw it on his forehead, and immediately re-vaccinated him. 21st.—A considerable crop of smallpox. Both vaccine marks seem to be taking. Notwithstanding, I again re-vaccinated, as his case seemed severe. 22d, 24th, 25th.—A great crop of pustules, confluent on face and forehead—severe. 25th.—Very severe; raving a good deal, and very restless. Vaccination aborted. 26th.—Easier; has got some refreshing sleep. Afterwards made a slow recovery.

Remarks.—In the first of these cases, re-vaccination took place a few hours after the eruption was noticed, and certainly seemed to be effectual in aborting the smallpox threatened. No doubt the attack was very mild, but still this must be classed distinctly as “aborted” smallpox; and the lesson it seems to teach is, that vaccination possesses some power in certain instances even after the eruption has appeared. On the eighth day, the few pocks on the body and the vaccination scratches felt like little scabs; and the question might be asked, Did the vaccination cause the abortion of the smallpox, or was the system so far protected as to abort both? On either supposition, the result was satisfactory; but nevertheless it is safe to give the patient the advantages that may be derived from re-vaccination, even in such cases. In the second case, the effects of re-vaccination appeared to me inappreciable. It seemed as if we had passed the boundary line to which the power of re-vaccination to restrain the disease extended, and that instead of the re-vaccination aborting the smallpox, the reverse took place. I may remark, also, that the same lymph was used to re-vaccinate this patient as in operating on the rest of the family, and that those of the latter succeeded, except in the father’s case partially—probably, as I have already remarked, from his being insusceptible.

13. The following little history of another family is so interesting, and seems to me so important, that I give it, though I fear at the risk of being tedious from repetition. On 7th May I was called to see Ellen

J., æt. 20, the smallpox eruption being already out on face and body. The affection was pretty sharp, and ran its usual course, the girl's face at the end of a fortnight having a good deal of the "Red Indian" look. I immediately recommended re-vaccination of all the children, and in doing this discovered a pock on the arm and another on the face of a lad, æt. 18. On inquiry I found, further, that on another girl, æt. 5, an eruption which had attracted little attention had been observed two weeks before this, she no doubt being the first that had introduced smallpox into the family. On 21st May, Isabella J., æt. 14 (who was re-vaccinated on 7th May), was in bed with very mild smallpox, which aborted. On 23d May, George J., æt. 21 (who also was re-vaccinated on 7th May, and has two small scabs on his arm), was in bed with sharp premonitory fever, and to make sure I again re-vaccinated him in four places. On 24th May, his mother called upon me late at night to say that he was very ill, the pain in his head being intense. She said also that she had noticed a single pock on his forearm. On 25th May, I found the patient much relieved, the pulse being soft, though a little quick. On examining the skin I found a good many red aborting pocks on forehead and arms, and on face, arms, and legs a densely crowded papular aborting eruption. The vaccine scratches are slightly irritated. 26th May.—Quite easy. Pocks and aborting papulæ beautifully seen to-day. 27th May.—Feels quite well, and wishes to get up; skin cool, pulse natural. One aborting red pock on forearm. The aborting papulæ could be counted by the thousand on face and arms. 28th May.—Has been up part of to-day, and feels quite well. The pock on forearm now forms a small scab; the papular eruption all but gone.¹

¹ In another case of a young woman, who had been re-vaccinated some months previously, I also noticed that, on the fifth day of the eruption, all the few pocks on the body had become minute scabs, except one on the forearm, which formed a small pustule, fully at its height. It would thus seem that while vaccination reduces the duration of smallpox by two days, re-vaccination does the same to modified smallpox, or aborts it altogether. Of the effects of vaccination immediately before a smallpox attack, I have seen only one instance, and perhaps that was not much to be relied on. On 31st May I vaccinated a child lying in bed with its mother, who had mild modified smallpox. On 1st June, as stated to me, the eruption appeared; and when I saw the child on 3d June, confluent smallpox covered face, limbs, and most of the body. I again saw it on the 5th, and on the 6th it died. On both occasions when I visited it, I looked narrowly for the vaccine scratches, but only confluent smallpox was to be seen; and yet it appeared to me that this looked more like a case of modified than of natural smallpox; for, 1st, The pocks were smaller in size than in the natural variety; and, 2d, On the fifth day of the eruption I observed that some of the pocks had already begun slightly to blacken. Is it possible that the introduction of the vaccine lymph into the system in such a case can modify the coming disease, although there is no local affection? Unlikely as this may at first sight appear, it, nevertheless, receives some countenance from the statement of Mr Mason, that in the London Smallpox Hospital the mortality among the "unvaccinated" was 35 per cent., while among those "stated to have been vaccinated, but having no marks," it was only 23·57 per cent.

Remarks.—This seems to me a very striking and instructive little history, and I can have very little doubt that, had the last case been allowed to run a natural course, we should have seen a severe case of confluent smallpox. We had both aborted pocks and papulæ. Had this anything to do with the re-vaccination on 7th May, and again, while in the premonitory fever, on 23d May? On 27th May I saw another case almost exactly similar to the last. A boy, æt. 11, slept in a bed close to his brother, who was very ill with confluent smallpox, and seems to-day to have pretty severe premonitory fever. He was re-vaccinated a fortnight since, and marks on arm are now small scabs. Again re-vaccinated him in four places. 29th May.—Up and going about, and has been out. Mother says nothing has come out; but, upon examining the skin, especially about the elbow-joints and calves of the legs, I showed her, much to her astonishment, that it was covered with a minute papular eruption, which appeared to me to be aborting smallpox. No pocks separate from the papular eruption were found in this instance.

The facts I have stated, which could easily be added to, and the experience we have had for some years, entitle us, I think, now to say, 1st, That even when quite properly performed, and all but universal, vaccination is not a security against the spread of smallpox among our population; 2d, That re-vaccination is a very decided preventive to the spread of the disease, and ought to be repeated every few years, and especially when a variolous epidemic threatens; and, 3d, That re-vaccination can not only prevent, or ward off, but can also “stamp out” or modify the disease after it has occurred, at least about up to the time when the eruption appears.

Assuming the existence of the evil, and the efficacy of the remedy proposed, to be granted, the question naturally presents itself, how the remedy can be applied. I have shown that considerable difficulties stand in the way, arising chiefly from the apathy, prejudices, and want of proper knowledge of the working classes. Parochial boards can and do compel vaccination, but they have no power to compel re-vaccination, and it is not to be expected that they will put themselves to much trouble in this respect, except in exceptional circumstances. By town councils, and their medical officers appointed to look after the sanitary condition of towns, a good deal might be done, if proper views were entertained, and the subject were taken up in earnest. Nothing could be more proper, I would say, than that, on the outbreak of smallpox in any locality, the authorities should immediately issue notices to all the inhabitants, recommending re-vaccination as a protection, and intimating also that the sanitary medical officer of the place would re-vaccinate *gratuitously* all who wished it, or were unable to pay for it. A great deal might be done in this way, I feel certain, to spread widely the protection referred to; and I do not see that either in

almost any other way could the sanitary medical officer make himself more useful, or that the working man need think there is any degradation in getting without fee what he helped to pay for in taxation.

With a very little more knowledge, and perhaps, in the meantime, some gentle persuasion, the children attending schools, too, might be re-vaccinated *en masse*, by the sanitary officer, or by their own medical attendants. I have observed that a fruitful mode of spreading the contagion has been from this source, and also in large works, and very likely in churches, and in other public assemblies, by those mildly affected or just recovering from an attack mingling with the unaffected.¹ Some very painful and even revolting cases of this kind have come to my knowledge. Managers of works co-operating with the medical attendants, or with the sanitary medical officer, could, with a little care and energy, I am quite convinced, do a great deal towards "stamping out" an epidemic.

Medical men, too, in their private practice among the working classes, should, I think, modify the fees very considerably for re-vaccination as compared with those for vaccination. As I have said, I have charged no fee for re-vaccination to my own patients who are members of my "medical clubs;" and evidently, in all cases where the disease has appeared, the surgeon's aim should be to think less of fees than to lend himself efficiently in "stamping out" this loathsome disease. Fee or no fee, I would say, whenever smallpox enters a house, let the patient at once, as far as practicable, be separated from the rest of the family, and let these, if

¹ I saw lately a case placing this danger in rather a new point of view. A lad from a little distance came to consult me. He stated that he had been seen previously by two other surgeons, one of whom had thought he was labouring under influenza, but that he had been ill for some nine or ten days, and that he was getting anxious about himself, as he was not improving. On looking at him I immediately noticed a papular eruption on his face and forehead, and one pimple on the right conjunctiva. Struck by its similarity to the cases I have been describing, I asked him if this was not smallpox? To which he replied that he did not know, but that something of the kind was over his body. On examining this, I was convinced that I had here a case of aborted variola, and my opinion was confirmed by the fact that he had been re-vaccinated two months previously. Now, this lad had been going about in railway-carriages and elsewhere for some seven or eight days, possibly throwing the contagion all round him, without exciting the suspicions of himself or any other person. From the greater prevalence lately of re-vaccination, cases of this kind will likely be much more common than hitherto, and even medical practitioners are apt to be thrown off their guard in diagnosing them. We now see smallpox and modified smallpox. In the future we shall also probably see a good deal of aborted smallpox.

Where the system has been well protected by the first vaccination, or by smallpox, I think it very likely that the aborting process takes place independently of re-vaccination, and without attracting much attention, and may therefore be easily mistaken or overlooked even by medical practitioners. Since I wrote the above, I have seen two cases I strongly incline to think were of this character.

possible, be re-vaccinated immediately. All possessed of the high moral tone which our profession inspires will revolt at the idea of a family surgeon quietly looking on at attack after attack striking down the members of a household, and perhaps infecting a whole neighbourhood, because funds have not been forthcoming to defray the expense of re-vaccination.

In England, the vaccinator's duties extend to the whole population. In Scotland, his services are required only when vaccination has been neglected. Perhaps with re-vaccination the English system might, to a certain extent, with advantage be adopted by us, if the requirements from medical sanitary officers in our burghs were properly defined and enforced. In August 1871 I had an intimation sent me that I had been appointed sanitary medical officer of this burgh, in the event of Asiatic cholera again paying us a visit, without any definite statement as to the duties to be performed; and I have since then, on several occasions when the outbreak of smallpox has been of a malignant character, on my own responsibility invited all the inhabitants in the neighbourhood to get themselves gratuitously re-vaccinated; but nothing has been done here or elsewhere, that I am aware of, in this respect on a systematic plan. If this short notice should be the means of directing the attention of our profession and public authorities to a sanitary question which seems to me of great prospective importance, I would fain hope that some good may be effected by it; and until re-vaccination, like vaccination, has become a law of the land (should that ever be the case), expedients of the kind suggested are perhaps all that are within our reach. It may not, however, be irrelevant to ask the question, Whether the time has not arrived for the General Medical Council, and our representatives in Parliament who take an interest in sanitary affairs, to move in this matter?

In conclusion, I have to say that, in all the cases I have cited, the re-vaccinations were performed in the usual way, by scratches, and that the lymph was obtained either from Dr Husband, or was taken from the arms of infants vaccinated by myself. I did not resort to the mode which has been described by Mr Furley, for the reasons that the method usually employed seemed quite satisfactory, and because, as I have already said, I saw that his mode had not proved successful in the hands of others. Notwithstanding that this may have been so, I must be allowed to say, that I think we lie under great obligations to Mr Furley, for having moved out of one of those grooves in which we are often all too apt to run, and for having suggested the possibility of breaking in upon the course of this dire disease; and I would just add, also, that perhaps, in the discussion at the Obstetrical Society, the acknowledgment of these obligations was a little too scanty.

Part Second.

REVIEWS.

The Mechanism of the Ossicles of the Ear and Membrana Tympani.

By H. HELMHOLTZ, Professor of Physiology in the University of Berlin. Translated from the German, with the Author's permission, by ALBERT H. BUCK and NORMAND SMITH, of New York. New York: William Wood & Co.: 1873. Pp. 69.

THIS essay, first published in *Pflüger's Archiv für Physiologie* in 1869, at once commands attention by the well-known difficulty of the subject of which it treats, and by the high reputation of the author as a mathematician, natural philosopher, and physiologist. It has rarely happened in the history of any science to have an investigator of such genius and high intellectual culture among its votaries. Physiology owes much to Helmholtz. The ophthalmoscope; the ophthalmometer, by which the difficult question of the mechanism of the accommodation of the eye to distance was finally solved; the accurate measurement of the rapidity of the nerve current; and the development of the wide subjects of modern physiological optics and acoustics,—are all more or less the work of this distinguished man. We are therefore desirous of knowing the results of Helmholtz's researches into the mechanics of the bones of the ear. The subject at present is treated loosely in text-books and lectures. We have for many years been satisfied with a vague notion that somehow these bones convey vibrations of the air to the labyrinth, but we were ignorant of the exact mechanism by which they do so, and of even their minute anatomical connexions. No doubt this ignorance may be accounted for partly by the intrinsic anatomical and physiological difficulties in the way of an investigation, and partly by the fact that few anatomists or physiologists have had that careful training in physics necessary for the successful prosecution of work of this kind. We quite concur in the remark made by the translators, in a brief preface, that this "is the only treatise in any language which enters fully into the anatomical, physiological, and mathematical aspects of the question, and will doubtless remain for many years to come the authoritative treatise on this subject." The essay is far from being easy reading. It is written in the stiff, condensed style peculiar to Helmholtz's scientific writings, and requires attention and study. It may therefore serve a good purpose to endeavour to assist those who have not time for its perusal, if we give a short abstract of the more important points brought forward.

Helmholtz first shows that, owing to the small dimensions of the

bones of the ear, and of the space occupied by the fluid in the labyrinth, as compared with the length of a wave of sound, the former must act as solid levers, and the latter as an incompressible fluid. This he demonstrates by mathematical reasoning; but the truth of the observation will be more readily apprehended by considering the case of the notes of the musical scale. The wave-length in air of C_1 (33 vibrations per second) is about 33 feet, while that of C_6 (4224 vibrations per second) is a little more than 3 inches. On the other hand, the dimensions of the ossicles and of the labyrinth are only fractions of an inch, or, in other words, are "fractions of the wave-lengths of those tones which we commonly hear, and which our ear can readily appreciate." The bones therefore do not vibrate synchronously with the vibrations of the air. If such a short chain could be caused to vibrate with regularity, tones would be produced so high as to be far beyond the range of the human ear. The bones act as solid bodies, receiving a series of shocks passing through them with such infinite rapidity as to be practically instantaneous.

We have next an elaborate anatomical description of the "Anatomy of the Membrana Tympani." Its attachments are given with a minuteness never before attempted. The most important point to be noticed is the form and direction of the membrane—a matter of interest in the mechanics of sound. The membrane is not stretched out flat on a ring (as in a tambourine), but its centre is drawn inwards by the handle of the malleus in such a way that the upper half of the membrane is in almost the same direction as the roof of the external auditory canal, while the lower half is "almost at a right angle with the axis of this canal." It is this perpendicular part which reflects back the light in aural examinations by a reflector. Helmholtz also points out that the membrane is almost inextensible, and possesses little elasticity. This fact any one can readily demonstrate by trying to tear it with needles.

The malleus has the following definite attachments by ligaments: 1st, by the handle to the internal surface of the membrana tympani; 2d, by a mass of tendinous fibres termed the *ligamentum anterius mallei*, which radiate from the root of the short process of the malleus (processus Folianus) to a sharp spine on the tympanic ring called by Helmholtz the *spina tympanica major*; 3d, by a strong round tense bundle of fibres, which forms the posterior border of the last-named ligament, and is therefore termed by Helmholtz, *ligamentum mallei posticum*; and, 4th, by a bundle of "satin-like" fibres which pass from a prominent ridge on the malleus to the sharp border of a recess in the tympanic ring, which is the last trace of the primitive visceral cleft, and is called the Rivinian recess. As this last-mentioned bundle passes externally, it is called the *ligamentum mallei externum*. These different tendinous fibres are so arranged as to permit the malleus to move only

round an axis, passing in a line parallel to the direction of the fibres of the *ligamentum mallei posticum*, and the middle fibres of the *ligamentum anterius mallei*. This line of fibres Helmholtz terms the *axis band*. The tension of these ligaments is such as to permit only of very slight movements of the malleus, even when unconnected with the incus. When connected with this bone the amount of movement must be extremely slight. The *tensor tympani* muscle has a double action as regards the malleus: first, to stretch the *membrana tympani* by pulling on the handle of the malleus; and, second, to tighten the axis band of the malleus, and thus assist in diminishing the extent of movement of that bone. The *laxator tympani* is not mentioned by Helmholtz, and we have been unable to identify it to our satisfaction with any of the ligaments above described. It is probably part of the *ligamentum mallei externum*.

The nature of the joint between the malleus and incus is next described, and it is shown that the handle of the malleus cannot rotate in the direction of the short process without carrying the incus with it. The hollow on the incus for the reception of the head of the malleus is like that of a saddle, but the one half of the saddle is larger than the other, and not only the convex but also the concave surfaces unite to form a ridge. When the head of the malleus is fitted into this hollow and rotated, it will be found, as we have determined by actual experiment, that the surfaces do not during the complete rotation maintain their apposition. If we rotate the malleus towards the short process the apposition is complete and the incus is moved; but if rotated in the other direction, the surfaces separate from each other and the incus is immovable. A capsular ligament surrounds the extremities of the bones; and indeed the whole arrangement bears an analogy to that described by Goodsir in his celebrated paper on "The Mechanism of the Knee-Joint."¹ Helmholtz also points out that the effect of the action of the *tensor tympani* muscle, with reference to the movements of the incus, is that, when it contracts, it places the malleus in such a position as to act on the incus with the greatest advantage so as to cause it to rotate and press against the *fenestra ovalis*. The movements of the stirrup next come under consideration. Helmholtz shows by ingenious experiments, and by calculation from the numerical results, that by condensing and rarefying the air in the external meatus, the displacement of the stapes amounted to only from .033 and .25 of a millimetre; and he states that "according to the highest calculation, the excursions of the stirrup amount to $\frac{1}{16}$ and $\frac{1}{8}$ mm." When we compare these fractions with the length of the wave in the most acute sound perceptible by human ears, it will be at once evident that this bone does not vibrate synchronously, but acts the part of a solid rod in transmitting vibrations.

In the next section we have a description of the "Concerted Action of the Bones of the Ear." Here we have the essence of the

¹ Anatomical Memoirs, vol. ii. p. 231.

whole work. Helmholtz shows that the system of the bones of the ear is that of a one-armed lever. The fulcrum lies where the point of the short process of the anvil presses outward against the wall of the cavity of the tympanum. The power is applied at the tip of the handle of the malleus, and the weight or point of resistance is at the point of the incus bearing the stirrup. The chain of bones, acting together because they are united by tense ligaments, is a lever of the second order, having the resistance between the power and the fulcrum. By calculation from measurements of the arms of the lever, Helmholtz shows that "the excursions of the tip of the handle of the anvil will amount only to two-thirds of that of the handle of the hammer, but the amount of the pressure which the former exerts on the stirrup will be $1\frac{1}{2}$ times as great as the force which is exerted against the handle of the hammer." The mechanism, then, of the bones of the ear is, that while it diminishes the latitude of the excursions or movements of the stirrup, it increases to a great extent the pressure or effect of these on the fluid in the labyrinth.

By cementing delicate glass threads to various points on the chain of bones, and causing these to move by condensing and rarefying the air in the external auditory canal, Helmholtz, by measurement of movement and calculation, has determined the amount of displacement of the individual bones. He has shown that the "excursion which the hammer can make without the anvil is nearly nine times as great as that which the two together can accomplish." He also states that when we render the membrana tympani tense by forcing air into the tympanum through the Eustachian tube, we hear the middle and upper notes of the scale more feebly than when the pressure on the external and internal surfaces of the membrane are nearly equal. He explains this by the fact, that when the membrane is tense the surfaces of the malleo-incudal joint are not in absolute contact, and therefore do not transmit movements so strongly. He states also that certain individuals occasionally hear a peculiar jarring noise from a tuning-fork in addition to the proper note, more especially in notes of high pitch, and that this noise at once disappears when air is forced into the tympanum. He attributes this noise to the rattling of the surfaces of the malleo-incudal joint on one another. When air is driven into the tympanum, the surfaces are separated and the noise is gone.

All of these facts described by Helmholtz may be illustrated by an ingenious model of the bones of the ear on a large scale, in which the ligaments are represented by strings passing round iron rotatory pins, so as to be tightened or relaxed at pleasure.

The next section is devoted to a mathematical and experimental investigation into the mechanism of the membrana tympani. The acoustic action of such a curved membrane has not been previously studied. He shows that the form of the membrane is such as to give it the greatest advantage with regard to mechanical work. Helmholtz has devised a simple apparatus which readily illustrates

this point. It consists of a simple monochord made of a string or wire stretched between two solid blocks of metal, and having a movable bridge. At right angles to the string and near one end of it, there is a short and wide tin tube, having the end next the string covered by a membrane of the form of the *membrana tympani*, the convex surface being directed into the tube. A light piece of wood is then placed between the membrane and the string. It will then be found, as we have proved by experimenting with the apparatus, that when we allow the vibrations of the string to pass through the rod to the curved membrane, the latter gives forth a sound much more intense than that of the string itself—indeed, as loud as that produced by drawing a bow across a string on a violin tuned to the same pitch. It thus intensifies the sound, and, as Helmholtz states, “this process resembles that which takes place in the *membrana tympani*, in so far as the curved membrane serves to transmit the vibrations from the air to a solid body of moderate weight and relatively small amplitude of vibration, as, for instance, the labyrinth-water on the one hand, and the ends of the strings on the other.”

The essay concludes with a mathematical appendix on the mechanism of curved membranes. It is altogether a great work in little compass. The only point not explained by Helmholtz is the necessity for well-formed joints in this chain of bones. No one who has examined the tympanum even after the disappearance of *rigor mortis* cannot but have observed how remarkably tense all the structures are, and how unlike vibratory structures they appear. The lever-theory, here propounded by Helmholtz, explains much; but there is still a difficulty in understanding why there should be a joint between the malleus and incus. In many of the lower animals there is no chain of bones, but only a single rod stretching from the membrane of the drum to the fenestra ovalis, and this transmits movements accurately. It is possible that the existence of joints may assist in the transmission of very fine shades of vibrations, of which many of the lower animals are unconscious; but of this there is still no experimental evidence.

A Study of Some Points in the Pathology of Cerebral Hæmorrhage.
By CH. BOUCHARD, M.D. Translated from the French, with
Notes, by T. J. MACLAGAN, M.D. Edinburgh: MacLachlan
and Stewart: 1872. Pp. 87.

M. BOUCHARD'S position as Physician to the Salpêtrière at Paris has given him opportunities of studying the pathology of cerebral hæmorrhage such as few other institutions in the world could afford. Of these opportunities he has fully availed himself; and the result is placed before the profession in England by Dr MacLagan in a small volume, which does credit both to the author and the translator.

The work is divided into four chapters. In the first, the influence of increased blood-tension as a cause of cerebral hæmorrhage is considered; and the conclusion come to is, that it plays "but a very limited part in the pathology of cerebral hæmorrhage." In the second, the effect of diminished consistence of the cerebral tissue is estimated; and the conclusion arrived at is, that it exercises only a very doubtful influence in the production of cerebral hæmorrhage. The changes in the vessels of the brain to which hæmorrhage has been ascribed, form the subject of the third chapter. In this the author points out the important part played by the lymphatic sheath, and shows how the recognition of its existence serves to reconcile facts, especially those observed in the study of fatty change in the minute cerebral arteries, which, without such knowledge, would be irreconcilable. The influence of atheroma of the vessels in producing cerebral hæmorrhage is also considered, and an important array of statistics, brought forward in support of the conclusion that "atheroma is not the essential cause of sanguineous apoplexy, and that it acts only as an aiding or predisposing cause."

The fourth chapter is the most important. Its title is, "On a hitherto undescribed Change of the Small Arteries of the Brain, as the most frequent Cause of Cerebral Hæmorrhage." This change "essentially consists in an exaggerated and often enormous multiplication of nuclei in the substance of the arterial coats, and in the lymphatic sheaths, and in atrophy of the muscular coat; . . . but the most important change is, that there are sometimes observed on a certain number of the arteries thus changed sudden dilatations, true aneurisms, the presence of which I have always succeeded in demonstrating in the brains of those who have died of sanguineous apoplexy—the hæmorrhage appearing to me generally to result from the rupture of these aneurisms."

It is these aneurisms, or rather the rupture of one or more of them, which M. Bouchard regards as the cause of hæmorrhage into the substance of the brain. This opinion he supports by a large number of interesting cases. From their size and appearance he has conferred on them the name of *miliary aneurisms*. They are all visible to the naked eye. "They look like little globular particles;" and a brain in which they are numerous is described as having the appearance of being dotted over with grains of sand. Sometimes only two or three aneurisms can be found; in others, a hundred or more may be detected.

For a full description of them, of the changes to which they give rise, and of the method of finding the exact seat of the hæmorrhage, we would refer our readers to M. Bouchard's most instructive brochure, a perusal of which will amply repay the reader.

It is proper to add, that M. Bouchard's work is the result of labours in which he was associated with M. Charcot.

The translator has added as an appendix a more recent joint paper on the same subject by MM. Charcot and Bouchard, in which

is given the result of their farther researches into the subject of cerebral hæmorrhage.

The translation is an extremely good one; the volume is very neatly got up; and the contents are illustrated by four well-executed plates.

The Pathology and Treatment of Diseases of the Skin. By J. L. MILTON, Esq., Senior Surgeon to St John's Hospital for Diseases of the Skin, etc. London: Robert Hardwicke: 1872. Pp. 358.

A Treatise on Diseases of the Skin and its Appendages. By AUSTIN MELDON, Esq., L.K.Q.C.P.I., etc. London: Longmans, Green, Reader, and Dyer: 1872. Pp. 270.

On the Treatment of Diseases of the Skin; with an Analysis of Eleven Thousand Consecutive Cases. By M'CALL ANDERSON, M.D., etc. London: Macmillan and Co.: 1872. Pp. 180.

Skin Diseases: their Description, Pathology, Diagnosis, and Treatment. By TILBURY FOX, M.D. etc. Third Edition. Re-written and Enlarged. London: Henry Renshaw: 1873. Pp. 532.

MR MELDON in his preface states in regard to skin diseases, that "very slight progress has been made in the pathology and treatment of these affections. The microscope has revealed the existence, in certain diseases, of vegetable and animal parasites; but beyond this we have not advanced." This statement sufficiently marks Mr Meldon's standpoint, and there is nothing in his book to show that he has attempted to improve upon it. The most interesting thing in the work is the collection of upwards of seventeen different classifications of skin diseases. The remarks on treatment are often useful, but marred for want of a distinct laying down of definite principles, which ought to guide us in the use of each remedy. His pathology is, he tells us, original, and is sometimes peculiar; otherwise the descriptions of the diseases are fair enough, and the book likely enough to be useful.

Mr Milton's work is of a different stamp, and contains the results of many years' experience in the treatment of skin diseases. It is full of clinical observations, is devoted almost solely to treatment, contains little in the way of systematic arrangement; and any remarks upon the pathology of the various diseases are by no means of that clear and precise character which we nowadays expect under that heading. His statements as to the non-parasitic nature of the tinea strike us as specially curious; and, though we are aware he has the high support of Erasmus Wilson in this view, we are sure no schoolmaster in Scotland will be found to believe in their non-communicable character; nor will any medical man here, we think, agree with him in regarding free purgation as an important element in the treatment of favus. This work is a very interesting

one, but is better suited for the study of the well-informed practitioner than for the hands of the student, unless he be an advanced student, of dermatology.

Dr M'Call Anderson is so well known as a successful cultivator of the science of dermatology, that every contribution from his pen upon this subject is always cordially welcomed; and this little work, containing the statistics of his experience of skin diseases, is peculiarly valuable, as it will enable dermatologists in all parts of the world to compare their statistics with those of Glasgow, and thus to a certain extent enable them to ascertain the comparative influence of cosmopolitan and of local causes in the production of such diseases. The second part, on the Therapeutics of Skin Diseases, gives us an epitome of Dr M'Call Anderson's experience, and is, therefore, also generally interesting, as enabling others to compare their experience with his; while a good index renders it very available for the busy practitioner, the author's name being a sufficient guarantee for the usefulness of the information conveyed.

But of all the works with which we have headed this review, there is not one which can compare, or was ever intended to compare, with that of Tilbury Fox. Though called a third edition, it is to all intents and purposes a new work, and we know of no other in the English language more fitted to be placed in the hands of any student of dermatology. Where we agree with him, which is mostly everywhere, we find his statements clear and accurate; and where we happen to disagree, we also find his statements so precise that there is no possibility of mistaking his views; and this is no mean excellence, if we look upon this volume as a basis for further advance, which every honest work ought to be. This book is copiously illustrated by very excellent woodcuts, representing various points in the histology of skin diseases, as well as many of the more interesting external appearances which are capable of being thus represented. Every one who has seen anything of skin diseases will acknowledge that Dr Fox's descriptions are most accurate, while all those at all acquainted with the pathology of those diseases will be equally ready to agree that his views are in accordance with those which are nowadays regarded as the most advanced, whether we agree with them or not; while, in regard to treatment, one of the wisest sentences in the whole book is that wherein he says, "I have attempted to show that it is impossible to formulate a particular treatment suitable for all cases of any one disease." Every one who thinks will at once acknowledge that the sole hope of future progress in the treatment or prophylaxis of skin or any other form of disease depends upon this—depends upon the due recognition of a variety of modifying influences, resulting partly from what are termed individual peculiarities, and partly from various external modifying circumstances, and also from various internal modifying circumstances, apart from constitutional differences, of which stage of the disease is perhaps the most important.

Dr Fox has a rather interesting chapter on eruptions, the direct result of the action of drugs—a matter with which it is of the utmost importance for every medical man to be acquainted; it seems entirely restricted to his own experience, at least we cannot otherwise account for his very meagre and peculiar account of the eruptions due to iodide of potassium, which he states “he thinks there can be no doubt” occasionally produces petechiæ, at other times boils or slight acne. We have never seen petechiæ produced by this drug; acne is, however, so frequent as almost to be regarded as an invariable result of the drug in full doses or continued for any length of time, and in some of these cases the further continuance of the use of the drug results in the formation of boils, which we do not remember having seen as an independent result of the iodide. But the most troublesome of all affections produced by iodide of potassium is a phlyctenular eruption which sometimes breaks out as large pemphigus-like bullæ, over the surface of the body, and at others as smaller and in many cases umbilicated vesicles; in every such case the fever is often high and the bodily distress extreme; no tolerance can be established, and the drug has to be given up. Where this idiosyncrasy occurs, iodide of sodium is not a bit more readily tolerated, often less so. The affection seems due to the iodine, and there is nothing for it but to give up its use. It is not a frequent affection—fortunately occurring not oftener than once in a hundred cases, perhaps not so often.

This work contains a very copious formulary, and a most excellent glossarial index, and is altogether a credit to its author and to the British school of Dermatology.

A System of Dental Surgery. By JOHN TOMES, F.R.S., and CHARLES TOMES, M.A. London: J. and A. Churchill

WE have already noticed, in favourable terms, the former edition of this work. The book is decidedly one of the leading treatises at present on the subject it discusses. All the best portions of its previous issue are preserved in the new one almost intact, and thus are excellent; while much that is useful, and not a little that is of scientific value, has been added by the author's son—the editor of the present publication.

In most works on Dentistry, more especially those of an exotic growth, and in the quasi-scientific papers contributed to our dental journals, there is often an obvious straining to appear profound. Wherever a Latin word or name can be introduced it is at once adopted, and the simple English discarded; hazy and often mis-conceived notions in physiology and pathology betray the writer's anxiety to exhibit his medical skill and acquirements; while the departments of medicine, surgery, and even midwifery, are some-

times invaded and dragged in—always with a Latin word or two—by those who too frequently show not only that they know nothing about such matters, but that they have no title whatever to meddle with them at all. Such is not the case with the work before us. It is simple, yet instructive; scientific, yet unpretentious. Since the former edition was published, papers on various subjects connected with dentistry, by men of our own country, have appeared. Some of these seem to have escaped the notice of the young editor. Continental theories are, no doubt, often deep and clever enough in many ways, but our own authorities should not be altogether ignored on that account. In the matters of chloroform, dental caries, constitutional affections connected with teeth, statistics of teething, syncope during dental operations, and many other papers, something certainly interesting, and possibly instructive, might have been found emanating even from Edinburgh, and sometimes given in the pages of this Journal. These and others have been apparently overlooked; but had some of them been referred to, it might have prevented the too positive assertion, and implicit adoption, of such views as that “caries is an effect of external causes, in which so-called ‘vital’ forces play no part.”

Constitutional Syphilis. By JAMES G. BEANEY, F.R.C.S. Melbourne: F. F. Bailliere: 1872.

THIS is a reproduction, with slight alterations, of a work which we had occasion to review three years ago, and which was entitled “Syphilis Popularly Considered.” Then we thought it our duty to speak unfavourably of the book, and as it is very little changed, except in name, we do not see occasion for changing our opinion.

Some of the passages which we had marked as objectionable in the former edition have been deleted, but many still remain. As examples of what we object to, we may state the following:—There are frequent disparaging allusions to the abilities and practice of brother practitioners, and of course the opposite is inferred of the author's powers. In fact, though upwards of a hundred cases are given throughout the book, we have not noticed one that terminated unfavourably, excepting a few quoted from other surgeons' practice. Mr Beaney seems to have a very poor opinion of his professional readers' knowledge, if we may judge from the want of technical phrases; or he may have mistaken to whom he was writing, for we find the following:—“One of the hints that may be judiciously thrown out for the better guidance of the *infected* in this matter is,” etc.

If Mr Beaney is to be believed, the greater proportion of diseases is due to syphilis, and the greater proportion of medical men either ignore this fact, or are incapable of recognising or treating syphilis.

The only thing we can say in favour of this book is, that it is a tolerable compendium of what has been written on the subject in this country and on the Continent, and may be of use to the medical practitioners of Australia, if they cannot get at the writings of Ricord, Divergie, Acton, Hill, Lee, etc., for themselves, and are content to wade through a great amount of chaff for a little good grain.

Memoir of Sir James Y. Simpson, Bart. By J. DUNS, D.D., F.R.S. Ed. Edinburgh: Edmonston and Douglas: 1873. Pp. 544.

THIS is a work which will be much read and much admired by many, most of all by those who knew not Simpson. Some also of those who knew him will accept it as a good portrayal of his life and character, for Simpson was many-sided, and apt to show to his friends the side that pleased them best. Others, however, will recognise, beneath a studied appearance of candour and fairness, a bias and a one-sidedness which completely pervert the lessons to be derived from Simpson's life. The author, being a Free Church professor, had other objects to serve than merely to write the biography of him who possessed one of the noblest intellects the present generation has witnessed; and there are also several passages in the book which distinctly prove that Sir James was too many for Dr Duns, and hoodwinked him, as he did many a better man; so that, not understanding his subject while alive, it is impossible he could do him justice now.

If there is one subject which more evidently than another shows this bias, scarce concealed by an appearance of impartiality, it is the history of Simpson's many quarrels; from which it would seem that almost the whole of the Edinburgh profession, and especially the best men in it, were from the first jealous of Simpson, contemned his lowly birth, and took every means to annoy him, and to keep him down; the reverse of all this being true. Simpson got his chair by one vote, and one vote was given him by the late Bailie Tait—a baker—because he was a baker's son; so that in a measure he owed his success in life to his plebeian parentage. Neither were his colleagues themselves of such patrician origin as to be able to regard him *de haut en bas*, even had they been desirous of doing so. Of all those who were professors when Simpson was elected, one alone remains—Sir Robert Christison, Bart.—and every one knows that he was not born in the purple, as he himself has told us that he is the first of his name to found a family in Scotland, while a very similar statement might be made in regard to all the others. Neither were the profession jealous of his brains. If his birth was humble, it was universally acknowledged that he belonged to the aristocracy of intellect; and pride in their countryman was a more

common failing than jealousy of his success. It is quite true that just at first his professional success did not seem to his colleagues quite secure. Accordingly another, and, as it then seemed, a better man, would, for the University's sake, have been preferred. There was therefore a tendency to cold-shoulder him, not because of hatred, jealousy, or contempt for the man, but because of over-weening love for the great interests at stake. This was a state of matters which a man of purer Christianity and loftier magnanimity would have let alone, and lived down; but Simpson was a man of war from his youth. Of strong feelings and strong passions, he was always in the midst of hostilities, always fighting, often against a mere chimera; and in this way he embittered his own life, and there can be little doubt shortened his days by the miserable squabbles arising out of the contest for the Principalship. What can be more contemptible—and with every respect for Simpson, that is the only applicable word—than the gratuitous charge made against Alison, in the letter at p. 123, that he had changed his hour of lecture simply to spite Simpson, and was rewarded by a very small class, at which Simpson chuckles in a manner which may be termed both inglorious and vainglorious? while every one knows that Physiology, of which Alison was then professor, is invariably taken at an early period of a student's career, and Midwifery not till a later, so that the two classes never can clash or interfere in the slightest degree with one another; and besides, as the class had to be taken by all intending graduates, a loss of a few students one year was necessarily made up by an increased attendance in a following session. Take it from either point of view, and there is little cause for chuckling to be found; and to charge Alison, of all men in the world, with acting in this paltry manner, is truly absurd. He had no feelings of petty jealousy, and all who knew him know that his was a truly religious life, in which every act was done with a single eye to the glory of God and the good of his fellow-men. Later in life, a number of unpleasantnesses which arose between Simpson and others, depended upon a loose and not very straightforward habit of his, which grew much upon him latterly, of saying pleasing things to all men, which led all parties to feel aggrieved, and think themselves deceived. The squabbles arising out of his last contest for the Practice of Physic Chair, are very prominent illustrations of this.

Very early in his professional career, Simpson's love of fame led him into the great mistake of attempting to do too much. The following letter from the late Dr John Argyll Robertson not only proves this, but gives sound advice, which, had it been acted upon, would have influenced for good all Simpson's future career:—

“Queen Street, 6th April 1846.

“MY DEAR DR,—Nothing but the high esteem and affection I bear you, and the deep gratitude which I feel for unbounded kindness and attention when sickness and death were present in my own family, could induce me to take up

the pen on a subject which must be very painful to you, and in which I have no right to interfere. I trust, however, to your considering my doing so as the act of a true friend. It is to point out to you the reports which are everywhere current of your inattention to patients who place themselves under your care ; that you neglect one set for the sake of another ; that you make appointments, keep patients in their beds waiting for you day after day ; that letters remain unanswered, and that patients are often left in ignorance whether they are to alter, continue, or give up the remedies you have prescribed ; and, in short, it is said by some that a patient cannot with safety entrust herself to your care from these causes. Now, my dear Dr, allow me to say candidly that in these matters you are not doing justice either to yourself or your patients, and that if even a slight foundation be given for a continuance of these reports in the public mind, no talents, however great, will ultimately bear up against them.

"Now, such instances of inattention must result from one or two causes : either that you have more practice than any single individual is equal to, or that there is a want of due arrangement. If the former, in my opinion you should decline taking charge of a portion of your patients, to whom you feel you cannot do full justice. If the latter, let your arrangements be changed. Whenever on leaving a patient you fix a day for again visiting her, enter it *at the moment* in your visit-book. Let it be at all times most correctly kept. Each day let a list of the visits be made out. If there be any which you find you cannot accomplish, let a note be *immediately* sent to the patient, intimating the circumstance, and fixing when you will call. If sent for unexpectedly to a case in which you are likely to be detained, let a message be sent to those expecting you. I am certain if you were to adopt some such method, you would do away with many of the current reports. If in the same manner you have not time to answer letters of consultation fully at the time they are expected, let a short note be written mentioning the circumstance. Now, my dear Dr, excuse me for stating the *absolute necessity* of some change being made. Your present mode must be injurious to your patients, and will ultimately be much more so to yourself. I have again to beg you to excuse me—that you will attribute my interference solely to the interest I take in all that concerns you, and that *I know* the advantage that is taken of these things by interested individuals, and those who have no kindly feeling towards you.—I remain, my dear Dr, ever faithfully yours,

"J. A. ROBERTSON."

Simpson's carelessness, though persisted in to the last, was not attended with the disastrous results to his practice here anticipated, though it had some influence even in that respect, and would have had much more had not Simpson been one of a thousand in ability, and so suave in his manner, that the most righteous indignation was apt to be melted before him. Nevertheless, this carelessness reacted injuriously on his own moral character, and is not an example to be held up for admiration, or even condoned or excused. There was no excuse for it, and it is a fault so perfectly inconsistent with true Christianity, that we are utterly surprised at Duns attempting to gloss it over.

Simpson's carelessness was exhibited in other ways than neglecting his patients. When money poured in so fast that it could not be spent, his carelessness led him to make rash investments ; and from one painful letter to Walter, we learn that he suffered for this. But if his carelessness led him into many dubious positions, Simpson's love of notoriety, and desire to be a leader in everything, from midwifery to Biblical criticism, conduced to place him in at least one position which, in his biographer's eyes, has atoned

for all shortcomings in other things. To other eyes it may seem that Simpson's "revival," which consisted not in any alteration in his habits of life, but simply in the use of certain forms of speech, and frequenting certain societies, in so far as it was not due solely to his desire to lead in everything, was merely a nineteenth century manifestation of the same spirit which induced his grandfather, a hundred years ago, to bury a cow alive to save his well-filled byre from devastation by murrain, and, a little later, prompted his uncle to set aside a "gudeman's croft," as an offering to the Spirit of Evil, to induce him for ever to abstain from blighting or damaging the rest of the farm.

This work is fluently written, reads well, and, as we have said, will be acceptable to most. We trust it may be a means of exciting a little more attention to the proposed monument, which has hung fire so long that we fear a new generation shall have arisen before it even takes shape; and yet one penny apiece from all who even loved and admired Simpson only to that extent, would raise a sum ample enough to honour even his great name.

A Treatise on the Continued Fevers of Great Britain. By CHARLES MURCHISON, M.D., LL.D., F.R.S., etc. London: Longmans, Green, and Co.: 1873. Second Edition. Pp. 729.

THIS new edition of this standard work requires no praise from us there are few equally comprehensive treatises on continued fevers in any modern language, and the profession has long since decided that there is none better. The present edition contains about a hundred pages of new matter, with many new diagrams and woodcuts. Hallier's theory of the parasitic origin of diseases, the views also of Beale, Chauveau, and Sanderson, as confirmed by Bastian, are carefully considered, and the subject of temperature in fevers is much more fully gone into than formerly. The subject of fever is therefore much more fully treated of than formerly, and quite brought up to our present standpoint.

A Practical Treatise on Urinary and Renal Diseases, etc. By WILLIAM ROBERTS, M.D. London: Smith, Elder, and Co.: 1872. Second Edition, revised and enlarged. Pp. 621.

THIS new edition of this very excellent work contains two entirely new articles, on Suppression of Urine, and on Paroxysmal Hæmaturia; both of them subjects of great importance to the practitioner as well as to the pathologist. The work itself is so well known as

to require no criticism ; it is quite abreast of our present knowledge of the diseases of which it treats, and may be consulted with confidence by both student and practitioner.

The Diseases of the Stomach: Being the Third Edition of "The Diagnosis and Treatment of the Varieties of Dyspepsia." By WILSON FOX, M.D., F.R.S., etc. London: Macmillan and Co.: 1872. Pp. 236.

THIS work is a very different one from the first edition of "The Diagnosis and Treatment of the Varieties of Dyspepsia," which was favourably reviewed at p. 484 of our Number for November 1867. It is now much fuller and more closely printed, and its information more condensed; so that, although it is actually enlarged by several new articles, especially by two on Ulcer and on Cancer of the Stomach, it yet contains fewer pages than the first edition. In these days of growth, when, as a rule, every edition expands till it almost bursts from mere repletion, this is no mean advantage, and it is no trifling compliment to the author to be able to say, that while it has actually decreased in size, like the Sibylline leaves, its value has increased.

A Handbook of Therapeutics. By SYDNEY RINGER, M.D., etc. London: H. R. Lewis: 1873. Third Edition. Pp. 576.

THIS third edition is considerably enlarged ; we have already had an opportunity of speaking favourably of former editions, and there is no need to retract our praises in regard to this. It is one of the few books which treat of drugs as instruments to be used, and not merely as members of the animal, vegetable, or mineral kingdoms incidentally possessed of certain virtues ; it is a work, therefore, pre-eminently useful to the busy practitioner, and its utility is greatly increased by an index of diseases, various remedies being classified under the heading of those diseases in which they are employed.

Posological Tables. By W. HANDSEL GRIFFITHS, L.R.C.P. and S. Ed. Dublin: Fannin and Co.: 1873.

THIS broadsheet is very useful for hanging in surgeries, etc., as a means of ready reference. It has already reached a second and improved edition.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LII.—MEETING IX.

Wednesday, 2d April 1873.—Dr MATTHEWS DUNCAN in the Chair.

I. *Dr Gamgee* showed a CASE OF PSEUDO-HYPERTROPHIC PARALYSIS in a boy aged 11. The patient had gradually lost the power of walking. The lower half of the body was evidently more developed than the upper half. This was specially remarked in the glutei and gastrocnemii. The disease consisted of an increase of the fat and cellular tissue, and not of the contractile elements of the muscles.

II. *Mr Annandale* read a paper on the OPERATIVE TREATMENT OF STRICTURE OF THE URETHRA. He pointed out the value of quinine in checking rigors, given in doses of from 5 to 20 grains. He divided permanent stricture into three varieties :

1. Simple organic uncomplicated.
2. Organic stricture with constant or constantly recurring spasm.
3. Complicated by (1) complete retention, unrelievable by catheter; (2) obliteration of urethra; (3) urinary infiltration and abscess and fistula; (4) calculus in bladder or urethra.

Operative procedures were divisible into—

- | | |
|----------------------------|-----------------------|
| A. Dilatation. | C. Internal division. |
| B. Splitting or rupturing. | D. External division. |

Simple uncomplicated stricture was best treated by gradual dilatation—rapid dilatation not being so satisfactory. Metallic instruments were better than softer instruments. A fine whalebone instrument was more useful, on account of its rigidity, than gum-elastic bougies. Splitting or rupturing were not advisable in this form of stricture, because, in his opinion, recontraction followed more rapidly than after gradual dilatation. Internal and external division were not necessary in simple uncomplicated stricture.

In organic stricture with spasm, Mr Syme at one time frequently employed external division. Holt's and Thomson's methods were both good, but the author recommended internal division on account of the immediate good result, and the complete disappearance of the spasm. He had operated on six cases. Five had been completely successful: the sixth died. In the fatal case, the patient, on admission, had frequent rigors, which he attributed to ague, from which he had suffered in Canada. The patient died six days after

the operation. The post-mortem revealed old-standing suppuration in the region of the prostate, and numerous false passages. There was a superficial clean cut into the stricture. The patient died of pyæmia, which he did not think was altogether due to the operation.

Mr Annandale showed various instruments used for internal division. He recommended an instrument which he had obtained from New York (*Dr Gourlay's*), which resembled the instrument recommended by *Maisonneuve*. An elastic guide is first passed through the stricture. A grooved staff is then passed, along which a triangular knife is directed, which cuts both from before backwards and from behind forwards, freely dividing the stricture. A silver catheter is then tied in for twenty-four hours, and a No. 12 bougie is passed occasionally afterwards. In all the cases the stricture was posterior; in none had he met with hæmorrhage or infiltration of urine.

In the worst forms of organic strictures with complications, *Mr Syme's* plan of external division was necessary, or a No. 10 bougie might be passed down to the stricture as a guide, and the urethra opened, the stricture searched for, a fine grooved staff passed from the opening into the bladder, on which the stricture could be freely divided.

Dr Bell highly approved of *Mr Syme's* operation. He did not approve of internal division; it was, pathologically, a bad operation. He believed in vital dilatation and external division. Internal division was unphilosophical and unsatisfactory.

Mr Annandale, in reply, stated that no plan of cure was permanent. The relief of spasm after internal division was analogous to the relief of spasm of the sphincter ani after division of a few fibres of the muscle. He had seen internal division followed by results which he had never observed follow any other plan of treatment. He had ample experience of all plans of treatment.

III. *Dr A. Gamgee* read a paper entitled NOTES OF A CASE OF INTERSTITIAL HEPATITIS, WITH ENLARGEMENT OF THE SPLEEN AND LYMPHATIC GLANDS, terminating fatally by hæmatemesis.

Drs Balfour, David Murray, Stephenson, Matthews Duncan, and the *Author*, took part in the discussion which followed.

SESSION LII.—MEETING X.

Wednesday, 21st May 1873.—*Dr HANDYSIDE*, President, in the Chair.

I. *Dr Tuke* showed TWO MICROSCOPIC PREPARATIONS.

1. Crystals of phosphate of lime on the inner surface of the pia mater. Cause of death, acute idiopathic melancholy.
2. Section of left corpus striatum, studded with minute aneurisms, from a patient whose right arm was paralyzed.

II. *Dr Stephenson* read a paper on CASES OF LARYNGEAL ABSCESS

SIMULATING CROUP. The author related the history of three cases of extra-laryngeal abscess in children which had occurred in his practice. He compared the symptoms of these cases with croup, and showed that the features distinguishing the disease from croup are :—1. The disease is shown in its progress. 2. The difficulty in swallowing is a prominent symptom. 3. The difficulty of breathing is increased when patient lies down in bed. 4. The cough differs from the true croupy cough.

Dr Joseph Bell referred to a paper of Mr Bickersteth's of Liverpool, and to a paper in the last number of the *Dublin University Magazine* by Mr Croley, entitled, "Acute Cellulitis of Neck." Mr C. believed that the disease commenced in the cellular tissue under the deep fascia, and that free and early incisions were necessary.

In answer to Professor Sanders, *Dr Stephenson* said that there was no necrosis of the thyroid cartilages in the cases he had met with.

The *President* related a case of a child four years old, in which he had been asked to perform tracheotomy for supposed croup. During the operation he discovered a large abscess behind and to the left of the cricoid cartilage. On opening it 4 ounces of pus escaped, with immediate relief to the symptoms. The patient made a rapid recovery.

III. *Dr John Duncan* read a paper on A NEW OPERATION FOR CLOSURE OF ARTIFICIAL ANUS. While the septum in general prevented the closure of the abnormal operation, there was no doubt that destruction of the septum did not always result in a cure. In April 1869, he operated on a femoral hernia on a servant girl. The bowel was gangrenous, and an artificial anus, which admitted two fingers, and through which all the fæces passed, was the result. In June 1870, no improvement had taken place. Dr D. passed a double silver wire through the septum, which was gradually tightened and worked its way out by ulceration. The fæces now passed per anum, but the orifice did not close. The operation was repeated in 1871 ineffectually. In April 1872, Dr Duncan pared the mucous membrane, and stitched the edges with catgut. He then pared the skin and brought the edges closely together with silver wire. A complete and permanent cure was the result.

The author claimed the following advantages for this method of treatment :—1. The normal mucous membrane is presented to the fæces. 2. The strain on the stitches is diminished. 3. The great depth of the rawed surface, by the adhesion of which the pressure from within is afterwards resisted.

Dr M. Duncan mentioned a case of femoral hernia in a pregnant female, in which an opening as large as the palm of the hand resulted in consequence of the sloughing of the bowel. After the birth of the child a gradual closure of the opening took place. No operation was required.

IV. *Dr Joseph Bell* read a paper on SURGICAL CASES, WITH SOME NOTES ON TEMPERATURE AFTER OPERATION, which appears at page 119 of our present number.

Drs Handyside, Sheaf, and John Duncan, and the *Author*, joined in a discussion which followed.

SESSION LII.—MEETING XI.

Wednesday, 4th June 1873.—*Dr HANDYSIDE, President*, in the Chair.

I. *Dr Argyll Robertson* showed a TUMOUR OF THE ORBIT, which he had removed along with the eyeball three weeks ago from a woman above forty years of age. The tumour was first noticed ten years ago. Its growth was unattended by pain, the patient's health was good, there were no glandular enlargements, and the walls of the orbit did not appear to be involved in the disease. The tumour was diagnosed to be a fibro-cystic growth, on account of an obscure feeling of fluctuation imparted by the tumour. The bleeding was excessive during the operation. The tumour measured above 3 inches from before backwards, and weighed $3\frac{1}{2}$ ounces. The eyeball was very much flattened from the pressure to which it had been subjected, but the coats of the eye were not invaded by the disease. The atrophied optic nerve could only be traced for a few lines into the substance of the growth. Microscopic examination—Spindle-celled sarcoma, with here and there nests of epithelial cells. The woman made a good recovery.

II. *Dr Joseph Bell* related the history of a case in which he had removed, from a woman aged thirty-four, a CANCEROUS TUMOUR OF THE LACHRYMAL GLAND, which implicated both the ocular conjunctiva and the periosteum of the orbit. To admit of free removal, the outer canthus had to be divided, and a plastic operation was necessary to fill up the gap left. The case did well.

III. *Dr Affleck* read a paper entitled, TWO CASES OF INTUSSUSCEPTION IN CHILDREN. The author, after relating the points of importance in the clinical history of intussusception in children, narrated the history of two cases in his own practice.

1. J. H., seven months; fatal result after a week's illness; post-mortem—lower part of ileum inverted into cæcum. 2. M. M., five months; recovery. In both, inflation was used. *Dr Affleck* believed it to be the remedy of most value in cases of intussusception in young children. To be of any use, it must be done at an early stage.

Dr Murray stated as his belief that the operation of gastrotomy for cases of intestinal obstruction was not justifiable.

Dr Sanders complimented *Dr A.* on the excellence of his paper. He stated that there were two varieties of intussusception in the

situation referred to in the paper; either—1. Ileum into cæcum; or, 2. Cæcum into ascending colon. It was in the latter variety that inflation would be especially useful. The circumstances were, in his opinion, exceptional and rare in which gastrotomy for intestinal obstruction was likely to be successful.

Dr M. Duncan considered that gastrotomy as a last resource added little to the danger, and would recommend the operation in selected cases. He believed that the pathology of intussusception would be aided by a study of the causes of inversion of the uterus.

Dr Gordon, Dr Cunynghame, Dr Chiene, and the Author, joined in the discussion.

IV. *Mr Annandale* then related the history of an operation he had performed in a CASE OF UMBILICAL HERNIA, WITH INTESTINAL OBSTRUCTION. The patient was a stout gentleman, measuring fifty-one inches round the waist, who had suffered for some years from an umbilical hernia the size of an orange. After a journey by coach he was seized with symptoms of obstruction. *Mr Annandale* saw him on the third day. The abdominal pain was severe, obstruction complete, with faecal vomiting. The hernial tumour was hard, but not tense or tender. There was redness around the hernia, a slough over the tumour, and the skin over it thin. Believing that, in these circumstances, it was dangerous to cut into the hernia, as he feared that further protrusion of the bowel would follow, *Mr A.* opened the abdominal cavity by an incision one inch and a half in length in the middle line, between the ensiform cartilage and the tumour. He then passed two fingers into the abdominal cavity, and easily drew out the incarcerated bowel from the hernial sac, and examined it. He then divided subcutaneously the sharp edges of the opening of the sac. In three hours the patient had a free evacuation. He improved for a time, but sank from exhaustion on the third day. *Mr A.* believed the cause of death to be a weak and fatty heart.

Dr Joseph Bell related two somewhat similar cases which had occurred in his practice with successful results. He approved of the method of operation in a similar case of umbilical hernia with slough over it.

Dr Chiene objected *in toto* to the method of operation, and thought that it would have been much better to cut directly down on the hernial tumour, divide the contracting neck of the sac, and return the bowel.

Part Fourth.

PERISCOPE.

ON THE MEDICINAL USE OF GREEN SOAP.—Mr J. M. Maisch, editor of the *American Journal of Pharmacy*, says (May 1873):—“A short time ago, a correspondent asked us for a formula for *tinctura saponis viridis composita*, which he informed us was used in the eastern cities. At first we were unsuccessful in our endeavours to comply with the wish of our correspondent, until Messrs William McIntyre and Gustavus Krause furnished us with the following formulas, which we publish below for the benefit of our readers.

“Commercial soft or green soap is usually made now, wholly or in part, from common whale and other fish oil. Hempseed oil, or rather a mixture of it with various other liquid fats, has formerly been used, but long since substituted by almost any refuse oil, which, on account of smell, rancidity, or colour, is unfit for other uses. These oils are saponified with caustic potash, and the desired green colour is imparted by blue, green, or yellow pigments, as may be required. Frequently, however, soft soap is met with of a blackish, or rather of such a dirty colour that it is difficult to distinguish a particular tint. The soft soap of the London Pharmacopœia of 1851, and of the Edinburgh Pharmacopœia of 1841, was directed to be made from olive oil and potash.

“Green soap is mainly used in medicine for the cure of itch, and in various other skin diseases, but, on account of its caustic nature, is not often applied.

“The following formulas have been furnished to us:—

“*Lotio Saponis viridis* (Prof. Hebra).—Green soap ʒj., boiling water Oj., oil of lavender ʒss. Mix.

“*Spiritus saponatus kalinus* (Prof. Hebra).—Green soap, 2 parts; 95 per cent. alcohol, 1 part. Scent *ad libitum*.

“*Tinct. Saponis vir. cum pice* (Prof. Hebra).—Green soap, tar, alcohol, equal weights of each.

“*Tinct. Saponis vir. comp.* (Tilbury Fox).—Green soap, oil of cade, alcohol, āā ʒj.; oil of lavender, fʒiss. Mix.”

PICRIC ACID AS TEST FOR ALBUMEN IN CLINICAL MEDICINE. By Mr GALIPPE. (Translated, with a few abbreviations, from the *Gazette Médicale de Paris*, 1873.)—I have often asked myself how it was that the most sensitive, and also the easiest test for albumen, picric acid, is not made use of. The various means employed to detect the presence of that substance require so much care that inexperienced persons are exposed to great mistakes. The

coagulating influence of heat on albumen is extremely precious, but one must not forget to render acid the fluid to be tested. And, if too much acid is added, albumen may lose its coagulability by heat. Nitric acid is lavishly used every day, but it may also lead to erroneous conclusions. If used cold, it decomposes the urates, and uric acid is precipitated, as it is only slightly soluble in an acid fluid, and it dissolves again only with the help of heat. Every one knows, besides, that at a high temperature nitric acid, with the greatest facility, converts albumen into xantho-proteic acid, which is easily dissolved if its quantity is small, or if the quantity of nitric acid is considerable. With Boedeker's test, the cyanoferride of potassium, there are also great chances of mistake. The use of the polarimeter does not seem to me to be at all practical in a hospital ward, unless a person especially familiar with the use of this instrument be charged with the duty of searching for albumen. Besides, small quantities could be recognised only by very few people. For the several years that I have used it, picric acid has appeared to me more advantageous than the other tests commonly used. It does not require either heat or acidification, and its colour admirably serves to detect even the most minute traces of albumen. I generally employ an aqueous solution saturated at the ordinary temperature of the air. This fluid, which contains but little picric acid, is amply sufficient to detect albumen, however small may be its amount in the fluid tested. In normal urine this solution never causes precipitation. An excess of the reagent never dissolves the precipitate formed. The *modus operandi* is very simple. A few cubic centimetres of the picric solution is put in a test tube, and then one or several drops of the fluid to be tested are allowed to fall in the solution. If there is albumen it traces a characteristic white line through the testing solution. This process deserves to be more extensively used. It suppresses many cases of mistake, which is a most important point.—*Archives of Scientific and Practical Medicine.*

EXTIRPATION OF THE WHOLE UTERUS IN A CASE OF INVERSION. By M. BARBA. (Translated from the *Gazette Médicale de Paris*, 1873.)—Barba employed first Chassaignac's linear ecraseur, and finished the operation by applying a metallic ligature. The extirpated parts clearly showed portions of the broad ligaments and of the Fallopian tubes, and the whole womb except a very small part of the os. The patient, after a slight peritonitis, left the hospital, cured, twenty-two days after the operation. Adding a similar fact of Courty and this one of Barba to the statistics given by West, a total of 61 ablations of inverted uterus is obtained, out of which there have been 44 cures. It is remarkable that ligaturing has given more cases of cure than the other modes of operation.—*Archives of Scientific and Practical Medicine.*

MEMORANDUM OF THE IDENTIFICATION OF BLOOD-STAINS. By Surgeon F. N. MACNAMARA, M.D., Professor of Chemistry,

Medical College, Calcutta.—Every medical officer should keep by him *blood samples*—pieces of cloth, wood, iron, which he has himself stained with human blood. These samples should be kept in a well-closed bottle, labelled with the date on which the stains were made.

The first test is the “solubility test.” The manuals on medical jurisprudence with one accord lay down the rule that the colouring matter of a blood-stain can be dissolved out by water, and if they do not state it so explicitly, they leave one to infer that a coloured stain is not of blood which does not colour the water in which for some hours it has been soaked. Undoubtedly, most blood-stains do give up their colour to water, but I have occasionally met with highly-coloured stains, unquestionably blood-stains, which gave no colour to water after soaking for twenty-four hours. Whether the damp heat of the plains changes the constitution of the colouring matter, or whether the dressing so freely applied to much of the cloth sent to India renders it insoluble, or whether it is some other cause which operates, I have not determined, but it is a fact that this solubility test must not be relied on as evidence against blood. It must, too, be borne in mind, that many suspicious stains are so small or faint that the solubility test, with its adjuncts, cannot be applied. However, most blood-stains, when placed in water for a few hours, yield up both albumen and colouring matter to it, and then such a solution is distinguished from other coloured solutions by its coagulability by heat and nitric acid, the accompanying destruction of the *red* colour of the *solution*, and by the action of a solution of soda, which changes the red colour to a greenish hue.

In addition to the “solubility test,” other two tests, as follows, must be employed; but these tests have this superiority over the other, that they may be used upon small faint stains to which it is inapplicable.

Soda Test.—A solution is made of 10 grains of caustic soda in an ounce of water, or, as the caustic soda of dispensaries has frequently to a great extent lost its causticity, double this quantity of soda may be dissolved, and then, after testing by means of the sample blood-stains, it may, if necessary, be diluted till, on addition to the blood-stain, it produces within a minute or two the characteristic reaction. This is a pale or dark olive green coloration, which passes at once into the previous red when acetic acid is added in excess, and again becomes green on treating afresh with soda. Long experience in the use of this test enables me to confirm Casper's assertion, that “there is no other colouring matter which undergoes these changes when thus treated.” The solution of soda and the acetic acid must be used sparingly, and any superfluous fluid may be removed with a piece of bibulous paper before applying the acetic acid or the soda a second time to the stain. Before making use of this test, let the soda solution be tried upon a sample stain, and also upon a part of the cloth under examination where no stain is apparent.

Guaiacum Test.—The operator will require tincture of guaiacum, which he should make from time to time afresh by dissolving a piece of guaiacum resin about the size of a pea in a couple of drachms of spirits of wine. He will also require some ozonized turpentine, which, I find, answers equally well with ozonized ether, while almost every sample of old turpentine will be found sufficiently ozonized for the purpose in hand. The operator must always try the guaiacum and the turpentine upon a sample blood-stain, and upon a part of the unstained cloth, before using it upon the suspected spot. On moistening the stain (a rather faint stain answers best by way of trial) with a drop of the tincture of guaiacum, and then applying a drop of the turpentine, a rich sapphire blue colour appears within a few seconds. Attention is not to be paid to the blue colour which will almost invariably form *after some interval* around the margin of the fluid circle.

In using the guaiacum test it is well, if the stain is an old one, to soak it for a few minutes in water, removing the superfluous water by means of a piece of bibulous paper prior to adding the tincture.

Stains on Iron Weapons.—The absence of blood may be established by means of the guaiacum test, thus:—Place a very small quantity of the rust on a white plate, add a single drop of water, and then with the end of a glass rod thoroughly rub down the rust, allow it to remain till almost dry, then add a drop of the guaiacum tincture, mix it with the moist rust, and quickly add a drop of ozonized turpentine; in the absence of blood, the blue colour will not appear, or will develop only after some interval. The operator should practise this test, using blood-stains taken from a rusty iron, and comparing the result with that obtained when no blood is present with the rust. I would not advise reliance on the guaiacum test alone as positive proof of the presence of blood in the rust; for it has been said that saliva, mucus, and other substances will produce the same effect as the colouring matter of blood. Further observations on this point are needed, but I may say that I have often tried saliva and mucus, and have never found the test respond to their presence. I suspect a little blood must have been accidentally present in the specimen which developed the characteristic blue colour.—*Indian Medical Gazette*, June 1873.

FRAGMENTARY REMARKS ON NEW AND OLD MEDICINE. By Surgeon-Major W. J. MOORE, L.R.C.P.—It is the fashion to assert that Englishmen are not the strong and healthy race they were. But of this there is no satisfactory proof. In former days, when the true born Briton lived without glass windows to his dwelling, when he surrounded it by a marshy moat or a fish-pond for the supply of his frequent fish diet, when he used fresh rushes instead of carpets or even boarded floors, when he drank strong mead, and ate (often stolen) venison, when his country was undrained and uncultivated, when dysentery and ague were endemic,

and when plague was a visitor, the weakly Briton died and the strong Briton lived. The giants of those days were, indeed, pretty much the remnant of the weakly—the survivors of a class, which class, under better public sanitary and personal hygienic conditions, now often live as long, if not as strong, as the more perfect type. The causes which killed them have disappeared, not before any marvels of medicine or any perfection of therapeutics, but as a consequence of the gradual amelioration of the conditions of life, and of the progress of sanitary improvement—still leaving, however, a larger number to the total of the population whose diseases would tend to the hasty conclusion of the change of type theory. Again, it will probably be admitted that the British soldier of the present day is, on the average, as good a man as his predecessors. His physique and vitality and strength have been proved, during recent campaigns, to be as good as such qualities ever were. Abyssinia, Burmah, the mutinies, have sufficiently proved this. But how great is the change of treatment which has occurred with regard to one of the most dangerous maladies from which he suffers—viz., insolation. Not so many years back bleeding was the remedy. Thus it is not so long since the assertion was made, that many patients would die without it (*Indian Annals of Medical Science*, vol. xii.), and even Morehead wrote in 1860 (*Clinical Research on Disease in India*, p. 621):—"An occasional case occurs in which cautious venesection might be useful." Now, it is generally admitted that the malady is one of exhaustion, and has no inflammatory action to be combated. Dr Hall, R.A., in a recent number of this journal, "On the Pathology of Heat Apoplexy" (*Indian Medical Gazette*, September 2, 1872), thus pithily sums up the views which most medical men now hold:—"For goodness sake don't let us revert to bleeding." But no one would probably assert that the type of sunstroke, or heat asphyxia, has changed. Such a malady must have been always the same, although for a long period it was confused with ordinary apoplexy, with remittent fever, with ardent or sun fever, with delirium tremens, or at least not recorded with the care of recent years. Neither would it appear, as above shown, that there has been any change of type in the soldier. Therefore we have at least one disease in which no change of type has occurred, acting on a class of men in whom no change of type has occurred, yet the same change has taken place in the treatment of this disease which has marked the progress of medical practice as regards the remedies applicable to other maladies. The change of type theory cannot, therefore, be a correct explanation of change of practice.—*Indian Medical Gazette*, June 1873.

Part Fifth.

MEDICAL NEWS.

CHAIR OF PHYSIOLOGY.—We trust that Dr Bennett may long be spared to conduct this important class; and we know of no reason why our London contemporaries should be so anxious to proclaim a probable vacancy, except to secure a priority claim for their special favourite among the candidates, among whom are mentioned Dr Rutherford, Dr M'Kendrick, and Dr Bell Pettigrew.

PROFESSOR GAIRDNER'S LECTURE ON THE LEGAL AND MEDICAL ASPECTS OF INSANITY CONTRASTED.—In his concluding lecture on insanity, Dr Gairdner took occasion to point out certain diversities between the medical and the legal views of unsound mind. Medicine looks upon insanity as a condition to be, if possible, cured. Law looks on it as a condition regulating the legality of certain procedures, the validity of certain documents, and the responsibility for certain acts. No legal or medical classification can be satisfactory which divides the population into two categories—the one sane and wholly responsible, the other insane and wholly irresponsible. Nature does not draw the line after this fashion, and if language or law does so, it must be at the sacrifice of truth, and, in the end, of justice. Even among the confessedly insane there are some who are at one time irresponsible, and at another time responsible in whole or in part, and the law admits this principle. Again, in ordinary criminal responsibility, the law admits a question of degrees of guilt, and, therefore, of punishment. The practical issue out of the requirements of the law as it stands at present has been that in such cases some juries, contrary to the ruling of the judge, have acquitted persons who were obviously criminals; while other juries have followed the ruling of the judge, and have returned a verdict against the prisoner in like circumstances, leaving it to the public to influence the Home Secretary, so as to secure an alteration of the sentence. Public opinion vibrates, in an almost incoherent manner, from one extreme to the other; but few authorities have maintained that it is *just*, no less than expedient, to consider even unsound minds as amenable to the law, up to the degree of their actual or ascertainable moral responsibility. A full recognition of this is necessary to preserve the moral sense of the community from being shocked by the spectacle, on the one hand, of the execution of a lunatic by mistake, or, on the other hand, of a criminal escaping punishment by some shadowy plea of insanity. Let evidence be led in every case as to the real nature of the criminal act, and let the jury form their opinion. If they are satisfied that the crime was the act of a

maniac, let them, as at present, acquit on the ground of insanity, giving in a verdict of "Not guilty, by reason of insanity." But let them have power also to return a verdict of "Guilty, but of unsound mind"—such a finding being understood to carry with it a mitigation at least, in all cases, of the extreme penalty of the law. After this, let a subsequent decision be come to by the judge, or by some other tribunal, as to the modified punishment proper to the degree of guilt, so far as it can be ascertained under the peculiar circumstances of the case; and, if necessary, let the decision be open to further appeal if, after a period of confinement in expiation of sentence, further evidence arises tending to bring into question the essential justice of any part of the punishment. In this way, and in no other, Professor Gairdner believes, will the real and most important services of science to criminal administration be secured, without the manifold extravagances and contradictions which at present seem to render them a mockery and a delusion.

ARMY MEDICAL DEPARTMENT.—Meetings of the Executive Medical Officers serving in Malta have been held, to discuss the effect on their present position and future prospects of the warrant of the 1st of March, and of the special circular of the 6th of March last. The proceedings of the meeting of medical officers held in the United Kingdom on the 5th of April last, that of the Parliamentary Bills Committee on the 18th of May, and the memorial of the Irish Medical Association, were before the meeting, who, after discussing the matter from every point of view, gave their unqualified approval to everything that had been advanced in these documents; they felt, however, that it would not be just to themselves or their comrades who were at home working for the general good, or to the civil members of the profession who were petitioning for justice to their military brethren, if they should shrink from laying a respectful statement of their opinions and wishes before the authorities. They were unanimous in condemning the intention of employing them as clerks and storekeepers, which they felt would place them in a false position, interfere with their proper duties as medical men, and prove therefore disadvantageous to the public service; at the same time they were agreed that, should the authorities insist on imposing on them these duties, for which they had neither special training nor aptitude, it would be their duty to obey cheerfully, so long as they continued in the service, which some intend to quit, should these objectionable duties be insisted upon, as soon as their private arrangements and circumstances enable them to take this step.

They decided on embodying their opinions in a respectful memorial, to be forwarded through the proper channel to the Secretary of State for War. They were unanimous in considering that improved retirements after twenty years' full-pay service, a certainty of promotion to the rank of Surgeon-Major at from twelve

to fifteen years' service, and the withdrawal of the special circular, were essential, and that less would not give satisfaction.

THE ARMY MEDICAL WARRANT OF 1ST MARCH 1873.—The following Memorial in reference to this Warrant has been addressed to the Director-General of the Army Medical Department, by the Royal College of Surgeons of Edinburgh:—"That the Royal College of Surgeons of Edinburgh, having had under consideration the present state of the Army Medical Department, have reason to believe that the Royal Warrant of 1st March 1873 is in several respects unjust and injurious to the medical officers of the army. That the College consider that only justice would be done if promotion were granted to surgeons after a stated period of service on full pay (say 12 years, as in the Indian Medical Service), and that this promotion should carry the pay of its rank, as under clause 3 of the Warrant a medical officer may now serve 15 years as surgeon, and even then, if not promoted, does not get the additional pay of 2s. 6d. per diem to which he would have been entitled as assistant-surgeon under the previous Warrant. That as medical officers are now simply attached to regiments for a limited period, their relative rank, as far as regulating choice of quarters is concerned, should place them on the cadre of the regiment or corps to which they are at the time attached, according to seniority from the date of commission, and not from the date of joining a regiment. That this rule should not apply to the commanding officer, who, of course, should always be senior. That the forage allowance hitherto made should not be withdrawn, or, if withdrawn, that a money compensation should be granted in lieu of it. That promotion from the rank of surgeon to that of surgeon-major should be, as stipulated by Royal Warrant of 1858, by seniority alone, excepting in the cases where proved inability forms a positive disqualification against the senior surgeon on the list standing first for promotion. That it would be but simple justice that the medical officers who had, previous to the passing of the Warrant, been gazetted to regiments, and paid the usual mess and band subscriptions, should have the same refunded to them, as, from the date of the passing of the Warrant, they are struck off the regimental list of officers. That, considering the special work of the medical officers, and the severe strain upon their constitutions in hot climates, where now so much of their service is passed, and especially bearing in mind, that when sickness is most rife, and the climate most unhealthy, and consequently their services in greatest requisition, they cannot get leave of absence as regularly as their brother combatant officers, the present scale of half-pay seems to the College so inadequate, that it will be in vain to expect that good men will be induced to join the service. That the College, in conclusion, humbly request that the Director-General will take these circumstances into careful consideration, with the view of

affording a speedy remedy for the grievances complained of.—
Signed in name and by appointment of the College,

EDINBURGH, 19th June 1873.

WM. WALKER, *President*.

KING AND QUEEN'S COLLEGE OF PHYSICIANS IN IRELAND.—The committee appointed to examine and report on the Army Medical Warrant of the 1st March 1873, having carefully considered the warrant in question, and the circular for the organization of the Army Hospital Corps, and a further circular, dated the 6th March last, beg to present the following report:—

The warrant and documents in question may be conveniently considered under the following heads:—

1. *The Unification and Consolidation of the Army Medical Department.*—On the policy of this change the committee do not consider themselves called upon to offer an opinion. They would observe, however, that one result of the measure is the omission of the names of surgeons attached to regiments or battalions from the list of regiments as published in the Army List; the effect of which must be to place the surgeons attached to regiments in an isolated position as regards the other officers, and to lower the social status they heretofore held in common with them. As a remedy for this defect, the committee suggest that, so long as any medical officer is attached to a regiment, his name shall be published in the Army List as a member of that corps. For pecuniary losses of every kind occasioned by the removal of the medical officers from their regiments, full compensation ought to be allowed, as in the case of combatant officers.

2. *Titles of the Medical Officers and their Relative Rank.*—These are regulated by articles 3, 5, 7, 8, 9. "Inspectors-general of hospitals" are in future to be styled surgeon-generals, taking rank with brigadier-generals, or after three years' service with major-generals. The committee see no reason why surgeon-generals should not rank as major-generals, and the director-general as lieutenant-general. Deputy inspector-generals of hospitals are to be entitled deputy surgeon-generals, and to rank first as lieutenant-colonels, and after five years' service as colonels. All surgeons of fifteen years' standing at the date of the warrant are to take the title of surgeon-major. They are to rank first as majors, and after twenty years' service as lieutenant-colonels, but junior of that rank. Surgeons who were formerly styled assistant-surgeons shall rank as lieutenants until after six years' service, when they shall rank as captains. In all advantages attached to those relative ranks respectively, the medical officers participate (article 3), "except that in the case of those who are attached to regiments, their choice of quarters shall be according to their regimental seniority." The committee recommend that every effort should be made to have the warrant modified so as that the right of the medical officers to choice of quarters shall depend upon his relative rank and the date of his commission.

3. *Promotion and Retirement* are regulated by article 12. From this it appears that promotion of surgeons of fifteen years' standing, and the increase of pay which accompanies it, will be contingent upon circumstances over which the officer has no control, and may be indefinitely postponed. Upon all such officers the warrant inflicts a positive injury. Articles 20, 28, 29, regulate the principles of retirement. A retiring pension of one pound a day for every year of full-pay service would have the desired effect of preventing stagnation in promotion. In any case retirement should be compulsory on full pay after five years' service in the rank of surgeon-general. Article 30 regulates retirement on full pay after twenty-five years' service, provided that the officer has served abroad, otherwise the pay to be but seven-tenths of full pay. This exception appears invidious. An officer may have been more efficient at home than abroad, and consequently the rule should be full pay for twenty-five years' service in any climate.

4. *Pay and Allowances*.—These are regulated by articles 3 and 4. Under the former warrant, assistant-surgeons of fifteen years' standing were entitled to receive 2s. 6d. additional to their pay. No provision has been made in the new warrant to secure a similar increase of pay to surgeons of the same standing. So grave a breach of contract would not be tolerated in the case of private employers. While on this part of the subject, the committee consider it their duty to point out that the pay of the army medical officers of the higher ranks is inadequate. The pay of surgeon-generals, late inspector-generals, is £2 per day, the same as it was fifty years ago; whilst the director-general, who is charged with the administration of the whole department, receives £1500, the pay of such officer half-a-century ago being £2000, with less duties and responsibilities. Art. 6 rules that "Forage shall be granted to officers of the Army Medical Department for such number of horses as are necessarily kept by them for duty." This regulation is in direct contradiction to the warrant of 1858, and to the recent regulations as to the allowance of forage to non-combatant officers.

5. *Military Hospitals*.—In the special army circular, dated the 6th March, regulating the organization of military hospitals, it is defined that, subject to the supreme authority of the governors and commandants of the hospitals, or of the general or other officer in command of the troops, the internal administration of those institutions shall be under the control of the Army Medical Department. With reference thereto, the committee submit that every effort should be made to abolish so obnoxious a regulation. The office of military governor or commandant ought to be filled in every case by a medical officer. It is inconceivable that an arrangement analogous to that now pointed out, which obtains in the naval hospitals, *e.g.*, at Haslar and Plymouth, and which the committee are informed works most beneficially, should not be adopted as regards military hospitals. Again, the circular charges the medical officer of mili-

tary hospitals with responsibilities of a wholly unprofessional character. So long, at least, as a military commandant or governor is attached to such establishments, duties such as those enumerated ought properly to be attached to his office.

In conclusion, the committee recommend that the College should use its best efforts to obtain such alteration in the existing regulations which apply to the medical officers of the army, as will effect the following changes:—1. The publication in the Army List of the names of all medical officers attached to regiments or battalions. 2. Full compensation for expenses or pecuniary losses occasioned by the removal of officers from those regiments or battalions. 3. Surgeon-generals to rank with major-generals; the director-general with lieutenant-general. 4. The retirement of surgeon-generals of five years' standing to be compulsory on full pay. 5. The pay of surgeons of fifteen years' standing to be 17s. 6d. per day. 6. The pay of senior surgeon-generals to be increased. 7. The pay of the director-general to be fixed at £2000 per annum. 8. Medical officers to have a right to allowance for forage, stable, and groom. 9. The military hospitals to be placed in the sole charge of medical officers.

Adopted by the College, 9th May 1873.

ALFRED HUDSON, M.D., *President*.

ROYAL EDINBURGH HOSPITAL FOR SICK CHILDREN.—At a special meeting of the directors of this Hospital, Dr Brakenridge, one of the extra physicians to the board, was appointed an ordinary physician in room of Dr Arthur Gamgee, who has been appointed Brackenbury Professor of Practical Physiology and Histology at Owen's College, Manchester. The vacancy among the extra physicians caused by the appointment of Dr Brakenridge has been filled up by Dr James Andrew, 52 Albany Street. Dr J. C. Renton has been appointed assistant to the extra physicians.

THE CHIGNON—A NEW FIELD FOR THE STUDY OF NATURAL HISTORY.—A medical friend reports for the *Sanitarian* the following case:—Being called recently to see one of his best patients, a matronly lady of about fifty summers, she greeted him—"Doctor, I want you to give me something for my head; it feels so, I could almost tear it to pieces, and it is all breaking out in sores." Examination revealed disease of the hair follicles and a high degree of irritation by the excessive use of the comb and brush to allay the itching. The nutrition of many of the hairs was evidently destroyed, and these dead hairs added to the mischief. Attention was next directed to the manner of dressing the hair—with what? Was the scalp kept too warm? "Nothing but soap and water, and occasionally a little bay rum to cool my head; and I wear only these light *rats* to keep my hair up, for you see it's getting very thin." On submitting a chignon fragment to the light of his microscope, he discovered numerous little nodes—or nests, as they

turned out to be—agglutinated to the hairs of the chignon for support and nourishment, each nest filled with eggs. These were in various degrees of maturity, some swollen and just ready to burst, as it were, under the genial warmth of the scalp, and each capable of establishing a colony of not less than fifty *psorosperms*—exceedingly small spherical animals, so light as to be wafted by the air, and adhesive to any congenial substance for nutriment. The patient was judiciously advised appropriate remedies, not the least of which was the discontinuance of the chignon, and she ere long recovered. M. Lindeman tells us that when these little creatures mature they are easily shaken from their resting-places, and, floating in the air, liable to penetrate into the interior of the human organism, reach the circulatory apparatus, and produce various maladies—cardiac affections, especially valvular disease of the heart, Bright's disease, pulmonary affections, etc. He calculates that in a ball-room containing fifty ladies, forty-five millions of psorosperms are set free; and he concludes that it is necessary to abolish false hair, which often proceeds from unclean persons.

PORTRAIT OF SIR WILLIAM FERGUSSON, BART.—It has been proposed by numerous friends and admirers of Sir Wm. Fergusson to open a subscription list, with the view of having his portrait painted by an artist of eminence. The portrait will, with Sir William's sanction, be offered to the Council of the Royal College of Surgeons, in the hope that it may be retained in that institution as a permanent record of one whose valuable contributions to surgical science, and whose social character, render him peculiarly worthy of so distinguished an honour.

Subscriptions will be received by Surgeon-Major Logie, Royal Horse Guards, Regent's Park Barracks, Albany Street, N.W.; and Charles Moss, Esq., Surgeon, 6 New Cavendish Street, Portland Place, London, honorary secretaries.

It is hoped that a replica of this portrait may be obtained for the Royal College of Surgeons, Edinburgh.

Scotch subscriptions may be sent to Prof. Spence, 21A Ainslie Place.

AIR AND LIGHT.—A Doctor's Story. By W. M. CARLETON.

I.

Good folks ever will have their way—
Good folks ever for it must pay.

But we, who are here and everywhere,
The burden of their faults must bear.

We must shoulder other's shame—
Fight their follies and take their blame;

Purge the body, and humour the mind;
Doctor the eyes when the soul is blind;

Build the column of health erect
On the quicksands of neglect.

Always shouldering others' shame—
Bearing their faults and taking the
blame.

II.

Deacon Rogers, he came to me;
"Wife is a-goin' to die," said he.

"Doctors great, an' doctors small,
Haven't improved her any at all.

Physic and blister, powders and pills,
And nothingsure but the doctors' bills!

Twenty old women with remedies new
Bother my wife the whole day throug'

Sweet as honey, or bitter as gall—
 Poor old woman, she takes 'em all.
 Sour or sweet, whatever they choose,
 Poor old woman, she daren't refuse.
 So she pleases whoc'er may call,
 An' death is suited the best of all.
 Physic and blister, powder an' pill—
 Bound to conquer, and sure to kill!"

III.

Mrs Rogers lay in her bed,
 Bandaged and blistered from foot to head.

Bandaged and blistered from head to toe.

Mrs Rogers was very low.

Bottle and saucer, spoon and cup,
 On the table stood bravely up;

Physic of high and low degree;
 Calomel, catnip, boneset tea:

Everything a body could bear
 Excepting light and water and air.

IV.

I opened the blinds; the day was bright,
 And God gave Mrs Rogers some light.

I opened the window; the day was fair,
 And God gave Mrs Rogers some air.

Bottles and blisters, powders and pills,
 Catnip, boneset, syrups and squills;

Drugs and medicines, high and low,
 I threw them as far as I could throw.

"What are you doing?" my patient cried.

"Frightening Death," I coolly replied.

"You are crazy!" a visitor said;
 I flung a bottle at her head.

V.

Deacon Rogers he came to me;
 "Wife is a-comin' round," said he.

"I re'lly think she will worry through;
 She scolds me just as she used to do.

All the people have poohed and slurred—

All the neighbours have had their word.

'Twas better to perish, some of 'em say,
 Than be cured in such an irregular way."

VI.

"Your wife," said I, "had God's good care,

And His remedies—light and water and air,

All of the doctors, beyond a doubt,
 Couldn't have cured Mrs Rogers without."

VII.

The Deacon smiled, and bowed his head;

"Then your bill is nothing," he said.

"God's be the glory, as you say!
 God bless you, Doctor! good day!
 good day!"

VIII.

*If ever I doctor that woman again,
 I'll give her medicines made by men.*

Muine Furner.

CORRESPONDENCE.

THE LATE PROFESSOR GOODSIR AND PROFESSOR VIRCHOW.

To the Editor of the Edinburgh Medical Journal.

SIR,—I hope you will kindly allow me to express, in a few words, my thanks for the truly seasonable and effective reclamation in behalf of the scientific claims of the late Professor Goodsir, as also of Professor Bennett and others, made in the last number of your Journal.

Only the day before the article in your Journal was brought under my notice by the kindness of a friend, I happened to see for the first time the latest edition of Kirkcs's "Handbook of Physiology," published this year. Now, from the first edition of this

work, published in 1848, down to that published in 1869, not only is Professor Goodsir's name made prominent mention of in connexion with the cellular theory, but it is also specially mentioned in connexion with the "Anatomical and Pathological Observations" in the long "List of Authors" at the end of the volume. Indeed, Professor Goodsir's name comes second in that list, immediately after that of Hunter. In the last edition of the work, however, his name occurs only once (in the body of the book), as part of the phrase "dental groove (Goodsir)." I need scarcely mention so mere a matter of course in the present state of the question between Professor Goodsir and the German professor, as that both the "Cellular Pathology" and its English translation by Dr Chance are most carefully registered after the name of Virchow in the "List of Authors."

I am perfectly well aware that, in the present phase of the cellular theory, the prominent question concerns the protoplasmic views of Professor Huxley on the one hand, and the "formed matter" of Dr Beale on the other. This, then, may explain why Professor Goodsir's name no longer stands so prominent in that part of the "Handbook of Physiology" which treats fundamentally of the cell-theory. But now that his Memoirs, as your article shows, have been collected and published in so excellent a form, through the liberality of the Messrs Black, it appears rather remarkable how the present editors of the late Dr Kirkes's work should have felt themselves called on to draw the pen through all the references that might—may I say ought to—have been made to them, with any needful changes, as formerly. I feel that this conduct on the part of these editors (I do not know their names) fully justifies all which I did and stated in 1868, when protesting against the admission of Professor Virchow among the Honorary Fellows of the Royal Society of Edinburgh; for I now witness him proclaimed in this book as, in fact, the only intellectual proprietor of what, to a most considerable extent, and that where the value is most fundamental, is not his. I say, "not his," on the strength of his own deed. That *systematic* work entitled "Cellular Pathology," he dedicates, in its English translation, to Professor Goodsir, as one of the earliest and most successful investigators of its subject; while, in this *systematic* book itself, he is named only in connexion (as your reviewer truly and forcibly points out) with one solitary and by no means important point. The conclusion from this is plain. The dedication and book flatly contradict each other; and the former, I hope, fully warrants me in again demanding, that what its author allowed to be Professor Goodsir's, be restored to him.

I have always felt that time would effect this just restitution, and now hail with gratitude your article, as a sure warrant of the justice of my expectation.—I remain, etc.,

JOSEPH TAYLOR GOODSIR.

P.S.—I may mention that I possess, among other documents of

my brother's, a number of notes on "Observations on the Development of Tissue," *Virchow's Archiv*, vol. xv. p. 465. In one of these, my brother plainly occupies the ground since taken up and defended so successfully by Dr Hutchison Stirling against Professor Huxley's disintegrating protoplasmic tenets. It is asked, in this note, Are cells not specific? In another of these notes, my brother claims various facts, and the principles involved in them, as having been ascertained by himself, but attributed wrongly in the *Archiv* to Virchow or others.

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*The Febrifuge Properties of the Cinchona Alkaloids — Cinchonia, Quinidia, and Cinchonidia*: Being the Abstract of a Thesis, for which a Gold Medal was awarded by the Medical Faculty of the University of Edinburgh, at Graduation on 1st August 1872. By JOSEPH DOUGALL, M.D., Surgeon, Madras Army.

WHEN the inquiry embraced in the following pages was commenced in 1866, medical men were little acquainted with the virtues of any of the alkaloids of the cinchona barks except those of quinia. No extended, comparative, and conclusive trials had been made on the effects in disease of the three other most important alkaloids—cinchonia, quinidia, and cinchonidia; which, nevertheless, are obtained so largely in the manufacture of quinia from some common cinchona barks, that the question of their utility had become one of great practical consequence. At the suggestion of the Principal Inspector-General of the Madras Medical Department, the required investigation was undertaken, after some years of experience in the usual treatment of malarious fevers in India, China, and the Straits of Malacca. But I had never tested there the physiological or therapeutic actions of the three alkaloids now referred to.

It is scarcely necessary to remark, that, in entering upon this investigation, I was entirely free from bias in favour of any one of them being more efficacious than another, or of any or all of them proving either more or less active and efficient towards the desired end than the sulphate of quinia. Throughout the whole course of the inquiry, I was influenced solely by the desire to give each of them a fair and carefully-conducted trial, all their effects being noted at the bedside of the sick at the time, and tabulated so far as the most important points were concerned.

The result is a series of facts, observed in several of the most malarious districts of the Madras Presidency, regarding the febrifuge virtues of the three cinchona alkaloids,—cinchonia, cinchonidia, and quinidia,—used in selected, well-marked cases of malarious fever, which were treated indiscriminately in rotation, and not

all with reference to the kind or severity of the symptoms, or any other circumstance.

In order to obtain an adequate supply of really well-marked cases of the most perfect type, I visited four of the most noted feverish places,—Russelkondah, Kimeddy, Chetterpore, and Jeypore; and there, certainly, I met with no difficulty in finding abundance of patients suffering from intermittent fever; the difficulty, indeed, being to limit the cases under observation at one time to such a number as could be observed with sufficient precision; for I was under the impression that a few cases very carefully watched would contribute more to the end in view than any number, however great, if imperfectly noted.

At the several places where my observations were made, there was either a civil dispensary, or a police hospital, or both. Although I could have had numerous cases of fever from amongst the general civil population also, I soon came to the conclusion that the majority of such cases would result in mere waste of time and medicine for my special object. Such patients were under no sort of discipline or restraint; were free to go whenever it pleased their inclination or suited their convenience; and were then beyond the reach of observation, or even of report. I therefore preferred taking most of my cases from public hospitals, where the men were under some sort of military control, and where more or less of a previous history of each could be obtained from official records, and no one under treatment could absent himself without authority.

I commenced testing the effects of the alkaloids under consideration at Russelkondah, which was at one time a military station, but had been abandoned for some years. It is now the headquarters of a considerable police force; and, though only a small town, it is the centre of a very wild district, inhabited by turbulent hill tribes, and requiring for its supervision a great amount of exposure and fatigue on the part of the constabulary. During March, April, and May, the heat is very intense and trying, so that there is no lack of cases of fever. In order to illustrate the treatment and its results, a few of these cases are here given, as selected from a much larger number, whose principal features have been given in a tabular form at the end of my Thesis.

CASE I.—*Treated with Quinidia.*

A medical assistant, stationed during the previous twelve months on the upper Godavery river, had been sent for twenty-one days into the hill tracts of Orissa, where he was first seized with fever. I saw him first on the morning of 13th July 1866, at seven o'clock. He said he had fever every other day for two months; that the paroxysm lasted about eighteen hours, sometimes twenty-four hours; that, while it lasted, there was great irritability of the stomach, with frequent vomiting, and intense pain in the region of the spleen; and that, prior to this illness, he had always enjoyed

good health. He was not anæmic. The skin was cold and moist; the pulse 84, small, equal, and regular; the tongue furred; and the appetite gone. The eyes were suffused, and the sclerotic slightly tinged yellow. The bowels were in a natural state, the urine scanty and high-coloured; the spleen was considerably enlarged, but the liver was of its ordinary size. He complained of nausea and frontal headache. Twenty grains of sulphate of quinia had been administered the previous day, before I saw him.

Five grains of quinidia were given every three hours during the intermission. Rigor occurred at half-past one P.M., when 12 grains were given. In half an hour, the pulse, from being small and feeble, became 124, strong, and hard; and the heat of the skin was great, and the eyes red and suffused. He complained of pain in the hind-head, and very much of pain in the spleen. This state continued unaltered till four next morning—the 14th; and then all the symptoms quickly increased. At seven o'clock he had repeated bilious vomiting and bilious diarrhoea, a warm, moist skin, the pulse at 96, small and hard, the tongue white; and he complained of nausea and headache, but much less of pain in the spleen. Five grains of quinidia were given. At noon he was free from fever. The skin was cool and moist; the headache and splenic pain were gone; the spleen was much reduced in size; and his only complaint was of nausea and weakness. Five grains of quinidia were again given, and again at five in the afternoon, when there was no change, except that he had three discharges from the bowels after noon. In the morning of the 15th he was free from fever and headache and sickness, but very weak, without appetite, and with a rapid, irregular pulse. The same dose of quinidia was ordered three times a day. He was sleepless and restless the succeeding night, but continued quite as well next morning—the 16th. There was no return of fever that day. The bowels were moved thrice. During the next two days he had no quinidia; but, in consequence of some return of pain in the spleen, he had three doses of the alkaloid on the 19th. On the 20th he was discharged well.

The quinidia taken in this instance amounted to 77 grains. It occasioned slight nausea after each dose. The bilious purging which followed it appeared to be caused by it; for, in the intermittent fevers of Russelkondah, there is not naturally any tendency to diarrhoea. The treatment lasted seven days.

CASE II.—*Treated with Quinidia.*

A police constable, nineteen years of age, a native of the hills, but resident for at least twelve months at Russelkondah, had repeated attacks of fever during six months prior to admission into the hospital on 15th July. He was not anæmic, but the spleen was slightly enlarged. In the morning of the 16th he had a frequent, weak pulse, a hot skin, a white tongue, nausea, no appetite, headache, and general soreness. Five grains of quinidia at seven

A.M. were followed by slight sickness, three bilious stools, reduction of the pulse, heat, and suffering. The same dose was repeated at noon, and at five P.M. He had four hours of slight fever during the subsequent night, but none next morning—the 17th; and his condition was in every way improved. Twelve grains of quinia were given in one dose. No further doses were given till the 20th, when he had slight fever from noon till seven P.M., and therefore got twelve grains of quinia twice. The bowels were five times moved after the first large dose on the 20th, but not next day; although twelve grains were again given for a slight return of fever in the afternoon at five. The medicine was not resumed, though there was a slight return of fever for two hours on the 22d. On the 25th he was dismissed well, but weak.

In this instance, as in the last case, the administration of the alkaloid was followed by smart diarrhoea. The quantity of quinia given was 63 grains during four days; and it was given indiscriminately during both paroxysm and intermission.

CASE III.—*Treated with Quinia.*

A police-constable, aged 20, who had been six months in the hills of Orissa, and suffered from fever almost continually during all that period, was admitted into the hospital of Russelkondah on 16th July 1866, affected with quotidian intermittent; anæmic, but free from enlargement of the spleen. He was severely affected with the usual symptoms of the paroxysm at half-past seven A.M., when I first saw him. Five grains of quinia given at this time, twelve grains more at noon, and a saline mixture at three P.M., every two hours, had no influence on the fever; which came to an end naturally at eight in the evening. Next morning at seven, while he continued free from fever, he had twelve grains more of quinia. This dose, as well as the first the day before, was followed by two loose discharges from the bowels. But the fever returned at noon, for four hours only, when twelve grains were again given, and again with the result of two loose discharges. Next day, the 18th, the fever again returned at noon; and it continued all the rest of the day, and the subsequent night, though the quinia was given again at noon in the dose of twelve grains, and with the apparent effect of again causing two loose discharges from the bowels. On the 19th the fever continued, and therefore twelve grains of the alkaloid were given in the morning, at noon, and at five in the afternoon. The fever did not come to an end till ten in the evening. On the morning of the 20th he was remarkably well; and he continued so till the afternoon of the 21st, taking only twelve grains of quinia once, at eleven in the forenoon of the 20th. At five P.M. of the 21st, as he was found somewhat feverish, twelve grains more were given. This attack lasted only three hours, and was slight throughout. A similar slight paroxysm recurred daily for two or three hours about five in the afternoon of the next six days; for which he had one

dose of eight or twelve grains every day on the formation of the paroxysm. The last of them was on 27th July. On the 30th he was discharged.

This was a very severe case, both by reason of its long continuance, the length of the paroxysms, and the vehemence of the fever. From an early period the quinidia seemed to shorten the paroxysm greatly; and then small daily doses brought the disease gradually to an end. Loose evacuations from the bowels followed the use of the remedy in the beginning, so long as the fever was severe, but not afterwards when the fit became mild and short. The quinidia taken amounted to 193 grains.

CASE IV.—*Treated with Quinidia.*

A police-constable, aged 22, from the hill district of Orissa, had suffered more or less from fever for a year, and when he arrived at the hospital of Russelkondah, on 17th July 1866, he was anæmic, had a greatly enlarged spleen, and had been suffering from fever daily for three days. At five in the afternoon he laboured under fever, commencing with rigors nine hours before. Twelve grains of quinidia were given at once. The fever ceased in four hours more. In the morning of the 18th he was free from fever, but had a sense of burning in the abdomen, nausea, and no appetite; and the bowels had been moved once. The dose of twelve grains was repeated; and on the fever recurring at noon that day, it was repeated again. On this occasion the fever lasted only three hours, during which there was a free discharge of bilious stools. As there was no fever on the 19th and 20th, the alkaloid was not continued; but on slight fever showing itself on the 21st, another dose of twelve grains was given, which was followed by three copious liquid bilious stools. The fever did not recur, and he was discharged on the 24th with the bowels slightly relaxed, and the spleen not reduced in size; but he was in other respects well.

At first the paroxysm in this case was severe and prolonged; but it was rendered milder and much shorter from the first administration of the quinidia; and bilious evacuations always accompanied the amendment. The quinidia taken amounted to 48 grains, and the duration of the fever under treatment was three days.

CASE V.—*Treated with Quinidia.*

A European military officer, aged 38, who had been previously much in the hill country of Orissa, and had sustained frequent attacks of fever for three years, came under my care on 20th July 1866, with quotidian fever. When I first saw him, at ten in the morning, his paroxysm had begun half an hour before with rigors, and was fully formed. The pulse was 104, full and strong, the tongue white, the skin hot and dry, and the bowels unaffected. He complained much of general soreness and headache. There was no anæmia, and no affection of the spleen.

Twelve grains of quinia, given at once, appeared to increase the headache; the bowels were moved twice soon afterwards, and the fit of fever came to an end, with perspiration. Next morning there was no fever; a second dose of twelve grains was given at eight; at ten there was a little increase of heat, but no other symptom of fever; nor was there any return afterwards, so that he was discharged on the 22d.

This was a case of prompt cure of simple intermittent, without any complication, by the administration of 24 grains only of quinia. As in the previous cases, the cure was attended with the discharge of bilious stools.

CASE VI.—*Treated with Quinia.*

A police constable, aged 20, who had been for some time in the hill tracts of Orissa, was received, on 21st July 1866, into the hospital of Russelkondah, labouring under quotidian fever, a fit of which he had sustained daily for eight days, commencing at four in the afternoon and lasting till midnight. When admitted at seven in the morning he had a foul tongue, but no fever; there was no anæmia, but a little enlargement of the spleen; the fever did not recur at its usual hour, when twelve grains of quinia were given; but it came on at midnight, lasted for three hours only, and was not so severe as usual. From this time he had no return of fever, so that he was dismissed on the 25th.

This was another instance of the prompt cure of an uncomplicated quotidian intermittent, without any of the usual bilious discharges from the bowels following the use of the quinia, of which only 12 grains were administered.

CASE VII.—*Treated with Quinia.*

A medical assistant from the upper Godavery river had been affected with quotidian fever for two months before admission into the hospital on 21st July 1866, in the afternoon. He was labouring at the time under a paroxysm, which had commenced at noon. The pulse was 128, the tongue white, the skin hot and dry, the bowels unaffected, the spleen much enlarged, but he had neither headache nor sickness, and was not anæmic. Twelve grains of quinia were given at five in the afternoon, but the fever lasted for eleven hours, during which there were several loose bilious discharges from the bowels. Next day he had a slighter paroxysm, and thrice took the same dose of quinia, with the same effect on the bowels. There was no fever subsequently, and no more alkaloid was required. For a day or two more he continued weak, and the bowels rather loose, but on the 26th he was well enough to be discharged.

This case occurred in the same person as the subject of Case I. He was seized on the day after he first left the hospital. The fresh attack was more easily arrested than the first. It was arrested in

two days by 48 grains of quinidia; and, as on the first occasion, the administration of the alkaloid was followed by copious bilious discharges from the bowels.

CASE VIII.—*Treated with Quinidia.*

A police constable, aged 33, who had been eight months among the hills of Orissa, and four months at Russelkondah, was admitted into hospital at four o'clock in the afternoon of 8th August 1866, immediately after the cessation of a twenty-two hours' paroxysm of quotidian intermittent, from which he had suffered more or less during eight months. The fever recommenced early in the morning of the 9th, when fifteen grains of quinidia were given. The bowels were slightly moved afterwards, and the paroxysm lasted only eight hours. On the 10th he had a slight paroxysm, without any rigor. On the 11th, although there was no distinct fever, the pulse was frequent and weak, and therefore ten grains of quinidia were resumed three times a day. The pulse continued frequent, but without retarding recovery, and the man was discharged on the 13th.

The total quinidia taken amounted to 56 grains. The spleen was not reduced in size. The bowels were moved only once a day.

CASE IX.—*Treated by Quinidia.*

A European, aged 27, superintendent of police, after suffering often and much from fever for three years, was compelled to go on leave to Europe for fifteen months. Four months only after his return to India he was attacked again by his old enemy more severely than ever. He was taken ill at a solitary rest-house, distant from headquarters, and had the advantage neither of treatment nor attendance for four days. The form of fever was a violent quotidian, but uncomplicated with either enlargement of the spleen or anæmia. I found him in a semi-comatose state on 18th October 1866. Thirty grains of quinidia administered at once caused subsidence of fever in an hour; and the subsidence was preceded by nausea, then griping, and then lowering of the pulse from 96 to 72, along with copious perspiration. Frequent loose bilious evacuations ensued during the first part of the intermission. Next day the paroxysm returned, and was similarly treated, with exactly the same results, and from that time the disease was arrested.

The potency of quinidia was well exemplified in this instance. Sixty grains, in two doses, checked in two days a quotidian of unusual severity, and the customary effect was observed on the bowels. Deafness attended the action of the remedy.

CASE X.—*Not arrested by Quinidia.*

A head police constable, aged 20, an inhabitant of Itchapoor, came under treatment on 8th August 1866. He had been suffering severely for eight days from uncomplicated quotidian. Quinidia at first seemed to have great effect in mitigating the paroxysms, and

between the fifth and eighth days of treatment he was comparatively well; but from the eighth to the thirteenth day the fever returned daily with increasing severity, in spite of the free administration of the alkaloid. It was given both in powder and in acid solution without avail, and also along with podophyllin. In ten days eighteen doses were taken, in all 255 grains, and on one occasion forty grains at once. The disease passed on the thirteenth day into the remittent type, and the treatment with quinidia was discontinued.

CASE XI.—*Treated with Cinchonia.*

A police constable, aged 25, had been for twelve months among the hills of Orissa, or at Russelkondah; and while among the hills he had been attacked with tertian intermittent, which soon passed into the quotidian form. When he had been three weeks ill, he was admitted into the hospital of Russelkondah, on 6th July 1866, and during the next seven days he had been treated with sulphate of quinia, of which, however, he had got only nineteen grains. The fever continued to recur daily and severely. He had slight enlargement of the spleen, anæmia, much weakness, and no appetite even during the intermission. On the 13th, during the intermission, he was ordered five grains of cinchonia three times a day. He had no fever all that day, nor the next, nor on the 15th, till five in the afternoon, when he had a paroxysm which lasted four hours. Twelve grains of cinchonia were ordered at the commencement of it. On the 16th there was no fever, and five grains of the alkaloid were resumed three days. On account of a slight attack on the 17th, he had twelve grains of cinchonia at once in the forenoon, and twelve more at noon. On the 18th he was well in the morning, though weak. The full dose of cinchonia was repeated as a precaution, and again at noon on the fever returning slightly. The bowels, which had been previously moved moderately after some of the doses, were now relieved by frequent loose bilious discharges, so copious that astringents were deemed advisable. At noon of the 19th he had again slight fever, and again twelve grains of cinchonia. On the 20th he had the cinchonia again, but scarcely any fever. On the 21st he had a slight paroxysm for the last time, and his last dose of cinchonia. On the 27th he was discharged. The spleen by this time was reduced to its natural size.

This was an unusually severe case. Nevertheless, the violence of the paroxysms was soon subdued, and in thirteen days of treatment a cure was accomplished. Two hundred and eleven grains of cinchonia were required, divided into twenty-four doses. As in the case of quinidia, the curative action of cinchonia was here attended with copious liquid bilious evacuations from the bowels.

CASE XII.—*Treated with Cinchonia.*

A police constable, who had been long in the hill tracts of Orissa,

and had there suffered from fever three months previously, was admitted into hospital for a fresh attack in the quotidian form, of four days' standing. He was a healthy young man of twenty, not anæmic, and without any enlargement of the spleen. When admitted on 14th July, he had, at seven in the morning, heavy headache, dry hot skin, a white tongue, a pulse at 88, and much nausea and want of appetite. He had five grains of cinchonia thrice that day. The fever left him at noon, and did not return till five in the afternoon of the following day, during which he had first two doses of five grains of cinchonia, and after five o'clock twelve grains. This paroxysm lasted only four hours. On the 16th he had three five-grain doses, and no fever. About seven of the morning of the 17th he had a slight attack, and got immediately twelve grains of cinchonia, and as much more at five in the afternoon. He had no subsequent attack, got soon well, and was discharged on the 21st.

This was an average sharp case, without any complication, at once mitigated by the alkaloid, and checked by 74 grains, given in eleven doses for four days. The bowels were slightly moved during the treatment.

CASE XIII.—*Treated with Cinchonia.*

A police constable, aged 45, from the hills of Orissa, was admitted into hospital in the morning of 16th July, on account of quotidian fever of fifteen days' standing. He was anæmic, had the spleen somewhat enlarged, and for three days had been suffering also from diarrhoea. Twelve grains of cinchonia were administered at once. Fever came on at five in the afternoon, the usual hour of attack previously, and lasted ten hours. Another twelve-grain dose was given on the appearance of fever. On the 17th the same dose was given in the morning, and on the fever recurring at noon it was repeated, with the effect of reducing its severity materially, and its duration to four hours. A very slight attack took place on the 18th and on the 19th, and a somewhat sharper one for a single hour on the 20th. One dose of twelve grains was given on each of the first two days, and two doses on the last. There was no fever on the 21st. On the 22d a slight recurrence of fever at noon rendered it advisable to repeat the cinchonia, and so also on the 23d. But there was no further return of fever, so that on the 27th the man was discharged.

From his anæmic appearance he must have been much longer ill than he stated. The complication of diarrhoea evidently retarded the action of the alkaloid; which on the other hand did not seem to increase the diarrhoea. Nine days of treatment were necessary, and 120 grains of cinchonia in ten doses.

CASE XIV.—*Treated with Cinchonia.*

A police constable, aged 28, from the hill district of Orissa, was admitted into hospital on 17th July 1866, for quotidian intermittent,

which for a whole month had attacked him regularly at midnight, and lasted about four hours. He was anæmic, but the spleen was not enlarged. He had his usual fit the previous night. Nevertheless it recurred at noon of the 17th, and lasted for four hours and a half. Twelve grains of cinchonia were then given. On the 18th there was no fever. Early on the 19th there was a short fit without rigor, and at noon a similar one, for which five grains of cinchonia were given thrice. A slight fit having again taken place in the early morning of the 20th, five grains were again given thrice that day. Next day he continued well, and was discharged.

This case was rather mild, but likely to be obstinate, as it had lasted long without interruption. Nevertheless it yielded readily to treatment. The paroxysms were at once mitigated; in three days they were arrested; and only 32 grains of cinchonia were required. The first doses were followed by abundant bilious discharges, and a slighter effect of the same nature was afterwards produced twice.

CASE XV.—*Treated with Cinchonia.*

A police constable, residing at Russelkondah, aged 20, came under treatment on 21st July 1866, in the second paroxysm of quotidian intermittent. There was anæmia, but no enlargement of the spleen. The fever had lasted twelve hours, and was severe. He got twelve grains of cinchonia at seven in the morning, and as much more both at mid-day and at five in the afternoon; but the fever did not subside till midnight, having thus endured for the unusual period of twenty-nine hours. It recurred at noon of the 22d, when the cinchonia was again given in the same dose. The paroxysm was milder, and lasted only seven hours. On the 23d he had a very mild attack, lasting, however, as long as the last. Twelve grains of cinchonia were given at noon, before it came on, and again at five, when it was forming. On the 24th and 25th he had no fever, and no more cinchonia; but on the 26th he had six grains on account of a slight threatening of an attack. On the 30th he was discharged.

The remedy here acted promptly on a very severe case, though of short continuance. The whole cinchonia used was 78 grains. Copious bilious evacuations from the bowels attended the progress of cure on the 22d and 23d.

CASE XVI.—*Treated with Cinchonia.*

A police constable, aged 20, residing in Russelkondah, who had not had fever for two years, was attacked severely on 21st July 1866, and again next day at noon, when he came under treatment. He was anæmic, but had not enlargement of the spleen. The second paroxysm lasted eight hours. He was ordered a saline mixture every three hours during the paroxysm. At six in the morning of the 23d the fit recurred, when he got twelve grains of cinchonia. In four hours the fever came to an end. On the 24th it recurred at three in the morning, and continued till two next morning, although

twelve grains of cinchonia were given three times. On the 25th he had again twelve grains; at five in the afternoon a slight fit, and then twelve grains more. On the 26th, as the pulse was frequent and irregular, though there was no fever, a final dose of twelve grains was given. Convalescence went on steadily, and on the 30th he was discharged.

This case was very similar in all its circumstances to the last. Eighty-four grains of cinchonia in four days put an end to a severe quotidian in its early stage, causing at the same time occasional bilious evacuations from the bowels.

CASE XVII.—*Treated with Cinchonia.*

A bugler, aged 22, who had been eight months in the hill district of Orissa, and subject to fever for six of these, was admitted into the hospital of Russelkondah in the fifteenth day of uncomplicated quotidian intermittent. He had slight enlargement of the spleen, but no anæmia. The fit recurred at ten in the morning of the 8th August 1866, the day of his admission, and again at ten at night, lasting severely for six hours. In the morning of the 9th he had fifteen grains of cinchonia. At five in the afternoon a three hours' paroxysm recurred, and fifteen grains more were administered. On the 10th, at the same hour, the fever returned without rigors, and lasted eleven hours. During this paroxysm twenty grains of cinchonia were given, along with half a grain of podophyllin. On the 11th, ten grains were given thrice a day. A paroxysm of five hours came on at five in the afternoon, without rigors, and not severe; much weakness was complained of afterwards on the 12th, when twenty grains of cinchonia were given. On the 13th and 14th, though there was no return of fever, ten grains were given, as the pulse continued frequent. On the 14th he was well enough to be discharged.

This was a rather obstinate case, as 125 grains of cinchonia, and six days of treatment, were necessary to arrest the fever. The usual action on the bowels did not take place. On the contrary, they continued on the whole constipated; and even when podophyllin was given with the alkaloid, the evacuations were deficient in bile.

CASE XVIII.—*Treated with Cinchonia.*

A house-servant, inhabiting Russelkondah, aged 27, came under treatment on 25th July 1866, for a simple tertian fever, which was arrested in three days by 48 grains of quinia. On 9th August the fever reappeared in the quotidian form at seven in the evening, and continued for nine hours. Ten grains of cinchonia were given next morning about seven. The fever returned at three on the following morning with severe rigors, and lasted rather severely for sixteen hours, during which he twice had fifteen grains of cinchonia. There was no return of fever afterwards, and he was discharged on the 13th.

This attack was less severe than the first, and it was more easily checked, by only 40 grains of cinchonia. The bowels were not acted on.

CASE XIX.—*Treated with Cinchonidia.*

A police constable, aged 24, inhabiting Russelkondah, who had been four days ill, was admitted into the hospital on 13th July, with an attack of uncomplicated quotidian fever, commencing regularly about one in the afternoon. He was ordered five grains of cinchonidia every four hours during the intermission. He had taken two doses when the paroxysm set in at its usual time, and it lasted six hours. On the 14th he had another attack, which lasted only four hours. The same system of treatment was continued. On the 15th he was attacked at four in the afternoon, the symptoms were milder, and the fever left him in two hours. On the 16th he had no attack. On the 17th it set in at six in the morning. The mode of administering the cinchonidia was then changed, twelve grains being given at once during the paroxysm, which again lasted mildly for two hours only. The same dose was repeated at five in the afternoon, as the pulse was frequent and the skin rather warm. The fever, however, never returned, and he was discharged well on the 21st.

The fever, a recent one, was readily enough mitigated and soon checked by the cinchonidia, of which 84 grains were taken in five days. The bowels were not acted on except once, and then slightly. The spleen swelled during the fit.

CASE XX.—*Treated with Cinchonidia.*

A sickly labourer lad of 15, an inhabitant of Russelkondah, had suffered for ten days from quotidian fever, commencing daily at five in the afternoon, and enduring severely for fifteen hours. He denied having ever had fever before; but this was rendered improbable by his very anæmic condition, and the enlarged and indurated state of the spleen. On the morning of the 15th July he got five grains of cinchonidia, and at noon the same dose. The paroxysm set in about five in the afternoon, and lasted, as usual, for fifteen hours, though twelve grains of cinchonidia were given soon after it commenced. On the 16th twelve grains were given about noon, and five in the afternoon, besides the dose in the morning taken before the fit of fever had come to an end. There was no regular paroxysm after this; but he had twice a twelve-grain dose on the 17th, once on the 18th, and a five-grain dose on the 19th, on account of some heat of skin and frequency of pulse. On the 23d he was dismissed.

The quantity of cinchonidia given was 87 grains in five days. In a rather severe case it acted promptly as soon as it was given freely. Bilious stools were occasioned, moderately at first, but freely during convalescence.

CASE XXI.—*Treated with Cinchonidia.*

A police constable, aged 18, residing in Russelkondah, had been affected with fever more or less for a month, and with quotidian intermittent for three days, when he came under treatment on the 16th July 1866. There was neither anæmia nor enlargement of the spleen. He was admitted while in a paroxysm, which usually lasted eight hours. Five grains of cinchonidia, given three times that day, were followed by copious bilious evacuations from the bowels. On the 17th the paroxysm set in at six in the morning, and lasted six hours. Twelve grains of cinchonidia were administered at seven in the morning, and again at mid-day. The fever did not return afterwards, so that the lad was discharged on the 19th.

This was an illustration of prompt cure of a severe uncomplicated quotidian with a moderate amount of cinchonidia, attended with free bilious discharges from the bowels.

CASE XXII.—*Treated with Cinchonidia.*

An hospital sweeper, of the pariah caste, aged 18, who had been for twelve months partly among the hills and partly at Russelkondah, was admitted into hospital on the 16th July 1866, for tertian intermittent. He had suffered from time to time for six months, and severely for ten days, before coming under treatment. He was very anæmic, and the spleen was much enlarged. His paroxysm usually commenced in the afternoon, and lasted eighteen hours, so that the intermission was thirty hours. He was ordered eight grains of cinchonidia every four hours, which occasioned frequent loose bilious stools. On the 17th he had a mild paroxysm at five in the afternoon, only four hours in duration; and the eight-grain doses were again given thrice. He had no more fever after this, and was dismissed well on the 21st.

This case exemplified the arrestment of a severe tertian promptly by 48 grains of cinchonidia, the cure being attended by copious bilious discharges from the bowels.

CASE XXIII.—*Treated with Cinchonidia.*

A police constable, aged 20, stationed for twelve months in Russelkondah or among the hills of Orissa, had laboured under quotidian fever for seven days, when he was admitted into the hospital on 17th July 1866, while under the paroxysm, which usually lasted fourteen hours, and was severe. Twelve grains of cinchonidia were given at once that afternoon, and next day, the 18th, twelve grains in the morning, and again at noon. The attack on the 18th was milder, and lasted only four hours. There was a little heat and increase of the pulse on the morning of the 19th, but no proper fever. On the 22d he was discharged well.

The energy of the cinchonidia was great in this case, for 36 grains arrested the disease entirely in two days. A gentle bilious discharge from the bowels attended the cure.

CASE XXIV.—*Treated with Cinchonidia.*

A Sepoy, aged 20, who had been twelve months at Berhampore, and had fever a year before the present seizure, was attacked at six on the evening of 20th July 1866, and came under observation next morning at seven, when the paroxysm still continued. Twelve grains of cinchonidia were administered at seven in the morning of the 21st at noon, and at five in the afternoon; but the fever did not pass off till midnight. Two bilious discharges were occasioned. On the 22d the fever recurred in the afternoon at five, when the cinchonidia was given once only. The paroxysm was mild, and lasted only three hours. A slight attack recurred at the same hour on the 23d, 24th, and 25th, and a very slight one on the 26th. On each occasion twelve grains of cinchonidia were given. There was no fever on the 27th; but the skin being rather warm and dry on the 28th, the man had five grains three times that day. On the 29th he was well, and was discharged.

In eight days 127 grains of cinchonidia were given. On the second day of treatment, with large doses, the duration of the fever, which turned out to be a quotidian, was reduced from thirty hours to three hours only. A single daily dose after that gradually arrested the disease. Bilious discharges occurred only when three large doses were given thrice during the first day.

CASE XXV.—*Treated with Cinchonidia.*

A police constable of Russelkondah, aged 25, anæmic, and with enlargement of the spleen, came under treatment on 23d July, affected with quotidian fever, of which two paroxysms had occurred before admission. He said he never had fever previously; but the condition of his spleen and blood testified to the contrary. Being in the cold stage at admission, he got at once twelve grains of cinchonidia, which occasioned three bilious discharges. The fit did not come to a head, there was no return of it afterwards, and he was discharged on the 27th, apparently cured by a single dose of twelve grains of the alkaloid.

CASE XXVI.—*Treated with Cinchonidia.*

A police constable, aged 30, who lived partly in Russelkondah, partly among the hills of Orissa, and had been liable to intermittent fever for eighteen months, was attacked with the usual symptoms on the 7th August 1866, about four in the afternoon; the paroxysm lasted twelve hours. At ten in the evening of the 8th it recommenced, and lasted eleven hours. On the morning of the 9th fifteen grains of cinchonidia were administered. The fever did not return again. On the 12th he was discharged, cured of a rather sharp quotidian by a single dose of fifteen grains of cinchonidia, without any observable action upon the liver or bowels.

CASE XXVII.

A Shikaree, aged 20, from Chittapore and the hills of Orissa, had quotidian intermittent for sixteen days about four months and a half before his present attack. He was neither anæmic nor affected with enlargement of the spleen. On the evening of 9th August 1866, he was seized with the usual symptoms of fever. When admitted into the hospital next morning the fever had ceased. Ten grains of cinchonidia were given. On the 11th he had a slight attack of fever, and again got ten grains of cinchonidia. There was no subsequent return of fever; and on the 13th he was discharged, cured of his fever, at its commencement, by twenty grains only of the alkaloid, without the bowels having been acted on.

The preceding selection is a fair representation of 108 cases of intermittent fever, which were treated at Russelkondah and its neighbourhood in July, August, September, October, November, and December, with one or another of the three alkaloids—quinidia, cinchonina, and cinchonidia—and carefully tabulated.

Intermittent fever, as it presents itself at Russelkondah and the adjacent districts of India, is frequently in the quotidian form. Of 108 cases which were the subject of my observations, only sixteen were tertians, two quartans, and two so brief, owing to the abruptness of their cure, that they might perhaps be considered as ephemeral fever. The remaining eighty-nine were quotidian intermittent. The only peculiarities I could observe in their character were the frequency of sickness and severe vomiting at the onset of rigor, and the rarity of diarrhoea. Diarrhoea was a concomitant in two instances only; in the others there was very generally constipation. In the Kimedya district the irritability of the stomach was generally absent, and the constipation of the bowels great.

In the trials made with the three alkaloids, they were used in succession indiscriminately as suitable cases occurred. Quinidia was given in thirty-nine cases, cinchonidia in thirty-five, and cinchonina in thirty-four. The sixteen cases of tertian were equally distributed among the three remedies. The whole 108 cases recovered, but in one the disease passed into remittent fever unchecked. In twenty-seven there was either no paroxysm at all after the first doses, or a very mild one merely, although the alkaloid was given during a single day only. In thirty others the treatment required to be continued only two days, and in fourteen others for three days. Thus, in seventy-one cases, or almost two-thirds, the fever was arrested within that period. In three only was it necessary to prolong the treatment to ten, twelve, and thirteen days.

There was very little difference between the alkaloids in the necessary duration of the treatment. But a difference was observed in the doses required. The average doses of quinidia varied between 31 and 70 grains, of cinchonina from 54 to 76, of cinchonidia from 43 to

90. Extreme cases are excluded from this calculation, because tending to vitiate the result. The conclusion is that quinidia is the most powerful, cinchonidia next to it, and cinchonia the least active; but that even cinchonia is energetic, and in an adequate dose a sure remedy.

The relation of these alkaloids to quinia in point of energy was not made the subject of express trial on this occasion. But prior observation justified the conclusion that it is equally active and certain in doses between two-thirds and three-fourths of those of quinidia. Quinia appeared more apt to aggravate headache during the paroxysm. It seems one of the best for preventing the tendency to relapse, when taken in small doses.

In the first trials with the three other alkaloids they were given during the apyretic interval, according to the usual rule. Ere long they were given indiscriminately during paroxysm and intermission. At length it became apparent that they were most serviceable when administered during the paroxysm only. The same observation has been lately made in various parts of India in regard to the febrifuge action of sulphate of quinia.

Quinidia appearing to be the most energetic of the alkaloids after quinia, some experiments were made on its physiological effects in the healthy state of the body. From trials on my own person, a dose of 10 to 15 grains, taken three hours after a light breakfast, occasioned severe nausea, afterwards slight griping, and in three hours one or two rather loose bilious discharges from the bowels. Twenty grains occasioned in twenty-five minutes so much nausea that vomiting could be restrained only by assuming the horizontal posture for an hour. In three hours three or four copious liquid bilious evacuations ensued. Twenty-five grains always induced vomiting, no matter what precautions were observed. These results will explain in some measure the physiological phenomena which often attended its curative action.

Of the usual phenomena of "cinchonism" observed during the free administration of quinia in intermittent fever, giddiness was the symptom most frequently noticed during the use of quinidia, as well as of cinchonia and cinchonidia, next ringing in the ears, next headache; and deafness occurred only twice. In twenty-six cases, however, there were no head symptoms produced at all. A more important concomitant of cure was bilious purging. This occurred indeed in fifty-four cases only, or exactly one-half of the whole. But the cure appeared to go on with greater speed and certainty when free bilious evacuations were produced. Accordingly, when the alkaloids failed to act on the bowels singly, as in the cases which occurred in the Kimey district, advantage was found in administering along with them a little podophyllin, or other cathartic. It was very rarely that the fever was not brought speedily to an end after bilious purging. This connexion indeed is easily understood, for it was seldom that there was not present some indication of congestion of the liver during the fevers of Russelkondah and the adjacent

districts, and this occasionally was well marked by the appearance of yellowness of the conjunctivæ. Reduction of the force of the pulse, and also of its frequency, was a common and early result of the administration of a full dose of these alkaloids. That effect was observed often when the remedy was given during the paroxysm, although the fever went on; and it was attended with much relief from the severity of the symptoms during what remained of the paroxysm.

The conclusions flowing from the preceding inquiries seem of great practical consequence, since it has been established by frequent recent experience that in various circumstances quinidia, cinclonia, and cinchonidia are largely obtained from several valuable cinchona barks, used by manufacturers for obtaining quinia, and that they are by no means the feeble, much less the inert, febrifuge remedies which at one time they were too hastily assumed to be.

ARTICLE II.—*On a Method of Operating in Certain Cases of Strangulated Hernia.* By THOMAS ANNANDALE, F.R.S.E.; Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery.

(Read before the Medico-Chirurgical Society of Edinburgh, 2d April.)

THE method of operating to which I wish to direct attention has, I believe, been previously suggested, but I am not aware that it has ever been practised on the human subject. For this latter reason I consider a report of the following case important. My friend, Dr Niven of Edinburgh, has more than once talked to me of the advisability of this operation, but it was only on meeting with the present case that I felt justified in performing it.

The principle of this method consists in making a small incision through the abdominal walls, and opening the abdominal cavity *near* the hernia, instead of cutting into the hernial sac itself, or exposing it. One or more fingers are then to be inserted into the abdominal cavity, and the protruded structures contained in the hernial sac drawn back from within.

The chief objections which may be reasonably urged against this proceeding are—1st, The risks likely to follow opening into the peritoneal cavity; and, 2d, that certain conditions of the hernial contents, such as gangrene, or firm adhesions, would prevent the operation being successfully carried out.

The first of these objections is not, in my opinion, very serious, but the latter is so, and therefore I would advise the method only in *certain* cases of strangulated or obstructed hernia.

The cases which appear to me to be most suitable for this operation are:—

(1.) Umbilical herniæ in the adult, especially if the coverings of the hernia are thin or ulcerated, or the hernia large.

(2.) Large inguinal or scrotal herniæ, which have been recently reducible.

(3.) Herniæ in other regions in which, owing to special conditions, the usual operation cannot satisfactorily be performed.

This operation is, however, only suggested when in these forms of herniæ the condition of strangulation or obstruction gives rise to symptoms which *absolutely* necessitate operative interference, and when signs of gangrene or a history of old-standing irreducibility are absent.

The incision into the abdominal cavity does not require to be more than from one to two inches in length, and it is quite possible, as I found in the case related, to examine the condition of the intestine contained in the sac, by carefully drawing it, bit by bit, through such a wound. If thought right, it is also quite practicable and safe to divide, subcutaneously through the same wound, any constricting band or agent which may be felt after the hernial sac has been emptied of its contents.

In the case reported, I was induced to perform this operation because I was aware that the usual operation is very unsatisfactory in cases of large and strangulated umbilical hernia.

The operation completely relieved the strangulation and obstruction; the bowels being freely moved shortly after. No symptoms of peritonitis followed the operation; but the constant vomiting, and the inability to take the slightest nourishment for some days before, together with a fatty condition of the heart, caused, in my opinion, the exhaustion which proved fatal.

CASE.—On the 23d of April last, Dr David Gordon asked me to see with him Mr B., æt. 61. The patient, who was very stout, and measured round the waist fifty-one inches, had suffered from a large umbilical hernia for many years. Three days before my visit he was suddenly seized, when travelling, with a pain in the abdomen, vomiting, and all the symptoms of *complete* intestinal obstruction. For two days he was treated at home by his friends, but on the morning of the third day he became alarmed at his condition, and sent for Dr Gordon, who, at once recognising the serious nature of the case, requested a consultation with me.

An examination determined the presence of an umbilical hernia, the size of a large orange. There was considerable inflammation of the superficial textures round the hernia, and a superficial slough of the skin covering the hernia itself. The hernia was not tense, but on laying hold of it its contents felt somewhat hard, and they were not reducible. The abdomen was swollen and tympanitic, and the patient had pain, stercoraceous vomiting, and all the other signs of intestinal obstruction well marked. Attempts to reduce the hernia were quite ineffectual, and an operation appeared to give the only chance of relief. The patient's consent to the operation having been obtained, and chloroform administered, I made an incision three inches in length in the mesial line, midway between

the umbilicus and ensiform cartilage; and after cutting through three inches of superficial fat, I carefully opened into the abdominal cavity, to the extent of about one inch and a half. A knuckle of gut at once protruded at the opening, and passing my fingers along the continuation of this portion of intestine, I found it passed into the hernial sac. The sac contained several inches (six at least) of this small intestine, which felt somewhat matted together, and also was adherent to the sac; the neck of the opening into the sac was slightly felt as a firm ring, but this ring was not very tight. Having determined these conditions with the finger, I gently drew open the intestine, which passed into the sac; and was pleased to find that the soft adhesions gave way and the whole contents of the sac were thus drawn out. As each portion of intestine contained in the sac was in this way drawn out, it was brought out through the abdominal wound and carefully examined. After the sac had been completely emptied, I introduced a probe-pointed knife through the wound, and freely divided, subcutaneously, the firm ring at the neck of the sac. The intestine contained in the sac was slightly congested in appearance, and on its surface were the remains of some recent adhesions, but there was no trace of any general peritonitis. The abdominal wound was closed by means of deep and superficial stitches, and antiseptic dressing applied.

Three hours after the operation, the patient's bowels were freely moved, and all symptoms of obstruction had disappeared. Next day he was weak, and the bowels were again slightly opened, but he complained of no pain or other bad symptom. The exhaustion, however, continued, and he died somewhat unexpectedly about forty-eight hours after the operation.

ARTICLE III.—*Case of Strychnine Poisoning successfully treated by Atropine.* By SAMUEL BUCKLEY, F.R.C.S. Eng., Visiting Medical Officer to the Prestwich Workhouse, etc., and late House-Physician at the Manchester Royal Infirmary.

E. P., æt. 28, was brought as an emergency case to the Manchester Royal Infirmary, in strong tetanic convulsions.

It appears, from her statement after recovery, that she purchased a packet of "Hunter's infallible vermin and insect destroyer," for the purpose of suicide. About four o'clock in the afternoon she took a pinch from the packet between her finger and thumb, placed it on her tongue, shut her mouth, and immediately got into an omnibus coming to Manchester. A sensation of dizziness and faintness soon came over her, quickly succeeded by a feeling of choking and dyspnoea, subsequently to which she lost all recollection of what occurred.

The omnibus arrived in Manchester at 4.25 P.M., from which she

was removed by the police to the Infirmary, the officer stating that he found her lying on the seat "as stiff as a board." When brought to the accident-room at 4.30 she was in a state of perfect opisthotonos, the spasms were severe and painful, and the intervals of exceedingly short duration. The stomach-pump was at once used (by Mr Boutflower, the house-surgeon), the stomach being emptied and well washed with water, after which she was put to bed between warm blankets, and a hot bottle was applied to the feet. Chloroform was administered to relieve the pain of the spasms, and at the suggestion of Dr Eason Wilkinson, atropine was injected subcutaneously, as an antidote to the effects produced by the poison. The liquor atropiæ (B. P.) was used, of which twenty minims ($\frac{1}{8}$ th of a grain) were injected three times at intervals of ten minutes. Under this treatment a semi-comatose condition supervened, and after each injection the spasms were observed to become milder in character. At this period the heart's action was impetuous and irregular, the impulse extending over a large area; no bruit was heard; the pulse was 130, and fluttering in character. Respiration was hurried and slightly stertorous. The pupils were widely dilated; the eyes had a peculiar fixed and bright appearance; the face was flushed, and during the earlier paroxysms the risus sardonicus was well marked, but had now disappeared, the features being natural but immovably fixed. The patient appearing to be well under the influence of the atropine, it was given more cautiously and at longer intervals in doses of $\frac{1}{4}$ gr. The spasms, however, increased in severity whenever the injections were long omitted. Chloroform was also discontinued during the intervals, which now lasted about twenty minutes, and given only during the attacks, which had become much lessened in intensity, the opisthotonos having entirely disappeared. The last severe spasm was at 8.30, after which there appeared to be more co-ordination in the muscular movements. Slight spasms, however, came on if the patient was touched or moved in the least, and a rather severe one was induced about 9.30 by the nurse changing the hot-water bottle. At 12.30 A.M. she began to show signs of returning consciousness. The pulse was 90, full and regular; respirations easy, and 22 per minute. She was with difficulty roused, and answered questions loudly spoken in a vague and muttering manner. After a time she swallowed a cupful of hot strong coffee, deglutition appearing painful. She fell asleep at 1.30, and shortly after was left to the care of the nurse. At 9 A.M. she suffered no inconvenience, beyond a feeling of uneasiness about the throat. The day following she complained of stiffness and pains in the joints. The urine was loaded with lithates for three days, and then became clear. She left the hospital, completely recovered from the effects of the poison, on the ninth day after admission, being detained a few days in consequence of a burn on the foot, arising from contact with the hot bottle, which, by the way, produced no irritation at the time to attract observation.

This is a case interesting, in the first place, on account of the well-marked antagonizing effects of atropine over the tetanic spasms produced by strychnine; in the second, that the large doses of so poisonous an alkaloid produced no ill effects, it being remarkable that no less than $1\frac{1}{2}$ grain of atropine (140 min. of the liquor atropine B. P.) was given during the case, and that $\frac{1}{4}$ th of a grain is known to have proved fatal in a case of poisoning by atropine. That the favourable results now related were due to the atropine solely, was evident by the immediate diminution in the number, duration, and intensity of the spasms following each injection, gradually regaining their strength after the lapse of a certain interval, each interval becoming longer as the case progressed. Although the chloroform was almost continuously administered during the earlier paroxysms, the mitigation of the symptoms only showed itself upon the injection of the atropine, the former appearing to have no effect whatever beyond the alleviation of the mental distress by the production of a partial unconsciousness.

The history gathered from her friends showed that she had twice previously attempted suicide, and had been in a low state of mind for some time, taking no interest in domestic affairs, and being perfectly regardless of surrounding circumstances; at the same time having an aversion to her children, husband, and all closely connected. I have since been requested to bear testimony that she was a proper person to be confined in an asylum. Hence the question may arise from the above history—Is a person in a state of melancholia less susceptible to the action of poisons in general, or to these in particular?

On examining the powder, a packet being obtained from the shop where she made her purchase, it was found to contain strychnine, cobalt, sugar, and starch (probably flour, as the powder under the microscope exhibited granules resembling wheat starch). A quantitative analysis was not made, for the reason that the amount of strychnine taken could only have been very approximately ascertained,—“a pinch” being a vague expression, and the remainder, after the *quantum sufficit* had been taken, was thrown away. The percentage must have been large, considering the effects produced, and that the contents withdrawn from the stomach thirty-five minutes after administration gave undoubted evidence of the presence of strychnine, showing it had not been all absorbed. The state of the patient on admission being apparently hopeless to all who saw her, the atropine was boldly pushed, and might perhaps have been considered to an unjustifiable extent, had the case terminated fatally; but the effects in counteracting the symptoms being so marked, and seemingly within control, its continuance was deemed perfectly warrantable, and met with the happy result here detailed.

ARTICLE IV.—*On the Comparative Advantages of Forceps, of Turning, and of Premature Labour in Contracted Pelvis: Illustrated by the Obstetrical Histories of Two Women.* By Dr ANGUS MACDONALD, F.R.C.P.E., F.R.S.E., Lecturer on Midwifery and Diseases of Women.

(Read before the Obstetrical Society of Edinburgh, 25th June 1873.)

THE following cases are laid before the Society with the view rather of originating a discussion upon a subject of the highest obstetrical interest, than with the expectation or assumption that they contain anything new or striking.

Learning some weeks ago, from one of our Secretaries, that the work on hand was rather scanty, I volunteered to relate them as being interesting in themselves, and likely to originate a profitable debate. I then expected to have been able to bring up the paper in some degree of perfection, but unforeseen circumstances have rendered it impossible for me to find the necessary time, which must be at the same time my apology for the crude form in which the paper is presented to the Society.

That the question raised is one of the most important and difficult within the range of our department, and one concerning which the opinions of obstetricians are very divided, is allowed on all hands. That the necessity of acting with a degree of selective precision between these various operations is needed, is also allowed on all hands. But the grounds for determining which is the proper method to adopt in each particular case, and whether any of these operations ought to be discarded from practice, is still a matter of great uncertainty.

Before, however, attempting any statement of opinion regarding these points, let me briefly go over the clinical history of these two women, whose cases are brought forward more as a theme from which to start the discussion, than as data on which to found deductions.

CASE I.—That of Mrs D. is peculiarly interesting. She is at present in exceedingly good health and spirits, buoying herself up in the hope that her next labour may be twin-boys, as her mother had twins once. Should this occur, she believes she may yet have a living boy, which is an object of great ambition with her. Her age is 41. She was married on the 10th of March 1854, since which time she has given birth to nine children at full term, to two at between 7½ and 8 months, and has had three abortions, which make in all 14 conceptions. She is of middle size, strongly made, and apparently well formed. Her pelvis, however, is very much contracted in the conjugate diameter of the brim, though roomy at the sides, and also roomy and regular at the outlet. The narrowing is effected by jutting forwards of the promontory of the sacrum, and is such as to contract it to close upon three inches, so far as one can

judge. The account I give of her confinements is, with the exception of the last three labours, obtained from her own statement, so that if there are mistakes in regard to any of them, some of the members of this Society, who happen to be referred to by her, will, I hope, put me right.

Her first confinement took place on 19th December 1854, when she was delivered of a female child. She was three days and three nights in labour. On the third day it was agreed upon between Dr Pattison, who had charge of her case, and Drs J. Y. Simpson and Sidey, to meet at 7 P.M., with the view of effecting delivery, provided the natural efforts were not successful before then in terminating the labour. She, however, gave birth to a living child, unaided, an hour before the appointed meeting. Her recovery was very slow, but complete. The child lived for several years, and died of hooping-cough.

This pregnancy was followed by two abortions. Her second confinement took place on 2d February 1858. Dr Sidey attended her. She lingered for a whole day, and Dr S. waited with her all night, till between two and three o'clock in the following morning, when Dr Keiller and he having chloroformed her, first attempted to deliver by instruments, but finally completed the labour by version; the child was a female, and is still living. Her third confinement occurred on 17th June 1859. Dr James Sidey, along with the late Mr William Scott Carmichael, attended. She was delivered by turning, under the influence of chloroform. The child was a male, and dead.

She was delivered by turning, under chloroform, of her fourth child, a girl, and living, by Dr James Sidey, on 19th March 1861. In the end of this year she had an attack of bilious fever, and in January 1862 was delivered of a female child, at about the seventh month. This infant survived only nine hours. The delivery was easy, and unaided. On 29th January 1863, Dr Andrew Inglis, assisted by Dr James Sidey, induced labour at the eighth month. In this case neither instrumental assistance nor turning was needed. The baby was a female, and though always weakly, lived till she was eighteen months old, and died in teething. The mother disliked the necessary interference, in connexion with the induction of premature labour, so much that she ever since declared that she would rather die than again submit to the same ordeal. In her seventh pregnancy she was allowed by Dr Andrew Inglis to go on to the full time, chloroformed, and delivered, by turning, of a female child, which lived. This happened on the 6th December 1865. On the 23d of May 1867, Dr Andrew Inglis again delivered her of a living female child, at full term, by version and chloroform.

On the morning of the 29th of June 1869, I was hastily summoned about 6 A.M. to attend her. She had previously insisted that she must be delivered by turning, and as Dr Andrew Inglis, who had handed over the case to me, was of the same opinion, it was agreed that on this occasion I should deliver her by turning. The os uteri was

completely dilated, and the water had escaped about half an hour previously to my arrival. The pains were excessively severe, yet the head had never entered the pelvis, and in the interval between the contractions was quite movable at the brim. I chloroformed the patient, turned with no difficulty whatever, and delivered her of a male child, which, however, never breathed. The promontory of the sacrum left a deep indentation on the child's head. Great force was needed to bring the head through the brim. Mrs D. made a speedy recovery.

On the 22d of October 1870, I was again called hurriedly to attend her. I found her in labour; the os fully dilated, the pains severe, but the membranes unruptured. I accordingly chloroformed the patient, and was proceeding to effect delivery by version, but on introducing my hand through the membranes, I found I had to deal with an ill-developed foetus floating in an excessive collection of amniotic fluid. On allowing this to escape, a small male foetus was expelled, which made a few faint efforts at breathing, and then died. On the 2d of January 1872, she had an abortion at about the third month.

On the morning of 28th of January 1873, she was delivered by me by turning, under chloroform, of a living female child. On this occasion also great force was needed to pull the child's head through the brim. The side of the child's head, which looked posteriorly, was deeply indented by the sacrum, and the consequent depression on the skull still remains to a certain amount. On this occasion I must confess that I was surprised as well as delighted when the child gave indications of life. The infant remains a healthy strong child.

CASE II.—Aged 25, was seized with labour pains on the forenoon of 22d March 1871. My then pupil, now Dr Shapter, of Exeter, sent for me late in the evening. On my arrival I found the os completely dilated, the pains very severe, but the head still stood over the inlet, and refused to enter the pelvis. The sacral promontory was very easily reached, and the whole pelvis—as, indeed, the woman's whole person—was remarkably small. After a little consideration, I thought that I should best succeed in effecting delivery by turning. Version was effected ultimately, but with some difficulty, owing to the spasmodically contracted condition of the uterus. The child had to be removed with the exertion of great traction force, and it took me nearly half an hour of interrupted pulling before I got the head clear of the pelvis. On an examination of the child after delivery, it was found that the connexion between two of the cervical vertebræ had yielded during the extraction. The mother made a good recovery.

From the want of success in this labour, it was resolved, with the patient's consent, to endeavour to bring home a living child in the next pregnancy by inducing premature labour. In this case my then pupil, now Dr Oswald Wood, had the general charge of the case. A condensed abstract of his report of it is as follows: "It

having been deemed proper to induce labour at about seven and a half months of pregnancy, on the evening of 20th April 1872, Dr Macdonald began the dilatation of the cervix by means of Barnes's gutta-percha dilators. Numbers 1 and 2 were introduced successively, and so far the dilatation advanced normally. But after the removal of No. 2, the cervix threatening to become spasmodic, it was deemed inadvisable to proceed to No. 3, and after introducing a flexible catheter between the uterus and the membrane and leaving it there, the case was left till next evening. At that time Dr Macdonald, accompanied by Dr Affleck, succeeded in effecting nearly complete dilatation of the cervix by No. 3 dilator.

"Pains by this time became established, but were much more of an irritating than of an expulsive nature; and as the head was, after waiting for an hour or two, making no progress, delivery was finally effected by the long forceps. The child came round and cried loudly, but died in about six hours. The patient on this occasion also recovered excellently."

This woman again becoming pregnant, and having a distinct aversion to a repeated attempt at saving her child by premature labour, I resolved to wait till the full term, and then attempt to deliver by forceps, leaving craniotomy or cephalotripsy as the alternative in case I failed with forceps. Accordingly, the patient having been seized with labour pains at full term, on the 4th May 1873, she was attended by my pupil, Mr John Bisset Smith. She had weak pains throughout the day, but these became exceedingly severe between seven and eight o'clock P.M. The membranes had ruptured immediately before ten P.M., at which hour I was called to attend her. I found the head presenting at the brim of the pelvis, with the sagittal suture approximating the transverse diameter of the inlet, with the occiput towards the left side of the mother. The head at this stage exhibited well-marked Roederer obliquity. At half-past eleven, as the head was making no progress, although the pains were long, strong, and frequent, and the cervix completely dilated, fearing rupture of the uterus in case of further delay, I applied long forceps, and after a pretty tight pull for the space of a quarter of an hour, succeeded in effecting delivery. The forceps were found to have been so applied that one blade was over the right eye, one limb of the blade being in a line with the ridge of the nose, whilst the other blade had become fixed behind the left ear, which proved the truth of the observation in regard to the transverse position occupied by the head at the brim. The child, a large, well-developed female, was semi-asphyxiated, but soon began to breathe under the ordinary restorative applications. The placenta, along with some clotted blood, was speedily expelled by the natural mechanism. There was for some time slight hæmorrhage. The patient and her child both did well.

The conjugate diameter was, on the strictest examination, made both by Dr Affleck and myself, after her second confinement,

determined to be about $3\frac{1}{4}$ inches; the diagonal conjugate, $3\frac{3}{4}$ inches.

The first remark I am inclined to make in reference to these cases is, that it seems to me that we are too apt, in Great Britain, in deciding as to the preference of one operation over another, to fix our attention exclusively upon the amount of contraction in the antero-posterior diameter, without, at the same time, keeping before us the presence or absence of contraction of the other diameters of the pelvis. Though there is no doubt that, generally speaking, though not always, the conjugate is the narrowest diameter in deformed pelves, yet, according as we have a contraction in the antero-posterior diameter merely, or general smallness of the pelvis, with or without a marked contraction of the sagittal at the brim, the operation of turning is followed with very different degrees of success. Now, the pelvis, in case No. I., presents to my mind a good example of the transversely flattened pelvis, roomy at the sides, but markedly contracted in the antero-posterior diameter of the brim. Such pelves, as they afford a considerable amount of room for the occiput to occupy in one or other of their sides, present favourable conditions for successful turning, while, at the same time, the narrow conjugate renders them somewhat less suitable for the application of forceps. In confirmation of this statement, it is to be observed that of all the three operations of forceps, premature labour, and turning, the latter gave the best results in this case. But No. II. being one of generally contracted pelvis, possibly, however, or rather probably unequally so, was very unsuitable for turning, whilst it gave an opportunity of effecting good results by forceps, and perhaps also better by waiting a considerable time before applying the instruments. The view that this pelvis belonged to the class of generally contracted pelvis, is made evident from the extreme flexion, or Rœderer obliquity of the head, which was noticed in the third confinement. Such pelves are exceedingly ill suited for delivery by version. This is prominently pointed out by Borinsky, *Archiv für Gynäkologie*, B. iv. s. 230. That British authorities, although no doubt aware of this fact, do not point it out with sufficient clearness, is, I think, unfortunate. At any rate, it was not sufficiently prominently before my mind three years ago, else I would certainly have preferred forceps to turning in the first confinement of Mrs N.

It is also very curious to notice how small an increase of the size of the child's head is found to be an obstacle of the most serious nature in cases of contracted pelvis. It will be observed that of Mrs D.'s children, not a male was able to be brought to the world alive,—the great variety in her medical attendants may fairly be held as eliminating all specialty in respect to relative skill in treatment,—whereas almost all the female children lived.

And now, a few words on the general proposition of this paper. In regard to the operation of premature labour in connexion with pelvic deformity, two very able statistical papers have lately been

published in Germany by Spiegelberg¹ and by Litzmann,² in the first of which it is broadly asserted that artificially-induced premature labour gives, in contracted pelvis, on the whole, very much worse results than allowing the cases to be terminated by the unaided natural efforts.

The results of Spiegelberg's collection of recorded cases of labour in contracted pelves are decidedly against the operation. The fatality in cases of contracted pelves generally he finds to be 6·6 per cent. of mothers, and 28·7 per cent. of children. The mortality, on the other hand, in artificially-induced premature labour generally, he finds 18 per cent. of mothers, and 66 per cent. of children; whilst the operation of premature labour undertaken for narrow pelves only gives a resulting mortality of 15 per cent. of mothers, and 66·9 per cent. of children. The percentages embrace a very large number of cases, from a wide circle of operators; the total number of labours with narrow pelves being 1224; of artificially induced labour for all causes, 271; of the same for narrow pelves, 219.

Litzmann, in the elaborate article already referred to, is inclined to believe that the results of carefully-selected and well-managed cases of artificially-induced premature labour ought not to prove so fatal to the mother as Spiegelberg makes out. But that the fatality to the children, taking into account not merely the number born dead, but the number who die within the first week or two after artificial premature labour, is quite as great as Spiegelberg makes it.

Of 373 cases of contracted pelves recorded by him, and variously treated, 6·9 per cent. of mothers, and 20·3 per cent. of the children died. Of the same cases, 34 of artificially-induced premature labour gave a maternal mortality of 14·7, and a foetal mortality of 55·8. He divides deformed pelves into four grades of contraction. His *first grade* of contraction includes—simple flat pelves with conjugate = 3·7—3·3 inches; generally narrowed, flat pelves with conjugate = 3·9—3·6 inches. Of these he records 316. Total mortality of mothers, 5·06; of children, 12·6. From artificially-induced premature labour, of which there were two cases, the maternal mortality was *nil*, but one of the children died = 50 per cent.

His second degree of contraction embraces—simple flat pelves, conjugate = 3·3—2·9 inches; generally narrowed, flat pelves, conjugate = 3·6 inches. Of 47 labours with pelves of these dimensions, the total mortality of the mothers was 10·6 per cent.; of the children, 61·7 per cent. Those deliveries effected by premature labour artificially induced gave a mortality of mothers 7·4, of children 51·8.

Classifying still according to degree of pelvic contraction, his results from 8 labours with pelves contracted to his third degree, which embraces—simple flat pelves, and generally contracted, flat pelves, conjugate = 2·9—2·2 inches. Total mortality of mothers, 50 per cent.; of children, 75 per cent. Of 5 cases of artificially-induced premature labour, 2 mothers died, or 40 per cent., and 4 children, or 80 per cent.

Of his fourth degree of contraction, which embraces pelves

¹ Archiv für Gynäkologie, B. i. s. 1.

² Lib. cit., B. ii. s. 16

a conjugate of 2·2 inches and under, it is not necessary to speak. Litzmann records only 2 cases, 1 in which delivery was effected by cæsarean section, and mother and child were lost; and another in which spontaneous premature labour came on, mother living and the child being lost. It would appear, however, from the statistical results both of Spiegelberg and of Litzmann, that spontaneously originating premature labour gives considerably better results in contracted pelves than when it is artificially induced. This result is obviously referable to irritation of the mother's parts by the various methods employed to bring on labour. It seems also evident to me that Litzmann's habit of practising, more particularly the method of intrauterine injection of fluid, has something to do with several of his unfortunate cases. Litzmann concludes as follows:—"The results of my investigations, as bearing upon practice, I should wish briefly thus to formulate. The artificial induction of premature labour in narrow pelves is offered especially in the interest of the mother; the saving of the child by its means is at least doubtful. The operation is, even in primiparæ, indicated in the second degree, and, at most, just at the commencement of the third degree of pelvic contractions" (*i.e.*, from 3·3—2·9 inches in conjugate); "in the first degree (3·9—3·3 inches conjugate) it can only be defended through the existence of specially difficult relations and complications."

It will thus be seen that while Litzmann—than whom no obstetrician in Europe is better entitled to speak in a matter involving contracted pelves—is of opinion that premature labour may give the mother a better chance than doing nothing, so far differing from Spiegelberg, yet the foetal mortality is simply enormous. Now, as premature labour has always been argued for, most of all on the ground that it gave the child a better chance, without bringing additional danger upon the mother, it seems to me that Litzmann's apology in its behalf is rather weak. There is a general impression throughout the profession that premature labour ought now to be much more seldom induced than formerly. I think it is a right one; and, from such records as I have brought before the Society, it seems to me that it were better for the mothers, and much better for the children, that the operation in connexion with deformed pelves were altogether given up. I do not profess to speak from much personal experience, but from reading and oral evidence. I have seldom found cause to employ it. This I do not regret; for, so far as all the information I can gather from reading and otherwise goes, and from the little practice I have had in it, it seems to be proved incontestably, that on very few occasions indeed is it the preferable operation. In one of the cases recorded by me, a weakly child was born, and lived some years, but succumbed to teething. I am not sure but in such, and in many other cases of death from children's disease, we are right in following the views brought before the profession lately by Virchow—that congenital weakness has much more to do than we usually think with such fatal results.

Thus, a child is said to die of hooping-cough, of measles, of teething, or of bronchitis ostensibly, but it really dies of congenital weakness, or of congenitally defective development. In the other instance, the infant survived its birth only a few hours.

But now, to turn very briefly to the comparative advantage of turning by the foot as an alternative to premature labour or forceps. This was argued to be, in reference to long forceps, a preferential operation by the late Sir James Y. Simpson, with his acknowledged ability and acuteness. Schroeder has also lately expressed himself as strongly in favour of the operation in the interest of the mother, especially, in contracted pelvis, even down to 2 inches 8 lines of conjugate.¹ Borinski, in the paper above cited, has discussed this question at great length, and with much ability. He points out with great precision the danger likely to arise from attempts at delivery by turning in generally contracted small pelves, and restricts it to the simple flat contracted pelves, and considers turning is more particularly required when the head presents at the deformed brim with lateral obliquity of the neck, so as to make the posterior or anterior parietal bone present with the sagittal suture running transversely, either forwards near the symphysis, or far backwards near the promontory of the sacrum.

My two cases illustrate the truth of Borinski's view. Case II. being one of generally contracted (flat?) pelvis, was of the worst shape to be benefited by turning. The fatal results of turning to the child, and the successful results of forceps, point, I think, to what ought generally to be our practice on these occasions. There must be in the recollection of every obstetrician of considerable experience a good few cases in which he had prepared his craniotomy or cephalotripsy instruments, and found, on applying forceps, that after a period of careful but vigorous traction, his efforts have been rewarded by a living child and a healthy mother. On the last occasion when Mrs N. was confined, I carried my cephalotribe to her house, in case forceps should fail. I well remember also another case in which I attended along with my then pupil, the late lamented Mr Thomas A. Peddie. It was that of a girl married at 16 and pregnant at full term, before she was out 17. On this occasion there was no deformity, but the pelvis was altogether too small, and the child was large. I waited a whole night and a day, but though the os was fully dilated, the head would not engage in the brim. I took with me also on this occasion my cephalotribe, etc., but determined to give the child a chance by forceps. To my astonishment, after a great deal of tugging, a living child was born. It did well, and so did its mother. Such cases are cropping up every day.

The cases in which turning is found to succeed best are such as my first case, in which we found on the whole that it did very well. But the great majority of such cases are made up of second and subsequent cases, which proved in the primiparous condition excessively difficult, and led to craniotomy and other serious results,

¹ Lehrbuch der Geburtshilfe, Dritte Auflage, s. 471-3.

as it is seldom in a first case that a difficulty suitable to be overcome by turning is proved to be so in time to allow of this operation being performed. These cases, however, are found to terminate just as easily by the application of forceps, if not without any interference at all. It is quite clear, on reading Schroeder's note after his paragraph on turning in deformed pelvis, that his views are arrived at chiefly from cases of great difficulty in first labours proving easy in subsequent labours, when turning was employed. But I hold that this reasoning is purely the *post hoc* fallacy. In the earlier part of my practice I turned much more frequently than now in such cases, and on the whole got fairly good results. Now I interfere much less readily, and find the results far better. I am not, however, inclined to hold that turning is such a purely harmless operation for the mother as Schroeder and others maintain. I can look back to two occasions on which I regret, for the mothers' sakes, that I did not employ forceps instead. I must also protest *in toto* against the views of Borinski, which are also to a certain extent shared in even by Schroeder and others, respecting the danger incurred by mother and child in using forceps. Such serious results may be true in employing those German and French instruments that are too long and approach too closely at the points, but are certainly not to be dreaded from the careful use of the English instrument, which is light and little bulky, and whose blades at their distal extremities do not approximate within an inch when out of use, and are much wider apart when applied.

Another misconception seems to have possessed men's minds in reference to the effect of the application of the forceps in increasing the tension of the already too small conjugate. Obstetricians persist in ascribing to the forceps the power of being a compressor. Now, every one possessing a knowledge of mechanics sees at a glance that the amount of pressure which is needed to prevent the forceps from slipping is very little indeed, and seldom, if ever, such as to interfere so injuriously with the progress of the head as Simpson and Schroeder will have them to do. If evidence on this score were needed, a single glance at such an instrument as Professor Inglis's long forceps, which I know to work well, ought to convince the most sceptical. The forceps is essentially a tractor, and nothing else, when properly applied and used, being able to exert the necessary traction with the slightest amount of compression, and mainly by friction of the blades on the child's head.

But without further discussing this question at present, I may conclude by stating that, so far as my own views of practice lead me, they are as follows:—

1. That, looking at the statistics of Spiegelberg and Litzmann, it seems exceedingly doubtful if the operation of inducing premature labour ought ever to be employed in cases of contracted pelvis.

2. That turning does not present any proved advantage to the

mother over long forceps in cases of contracted flat pelvis (so far at least as my experience and my reading enable me to form an opinion), and is undoubtedly more dangerous to the child. That it is entirely unsuitable when the contraction is general, being much more dangerous to the mother than long forceps, or any of the higher operations.

3. That in contracted pelvis, as a general rule, it is on the whole safer to let the case go on to the full term of utero-gestation, and then give the patient a fair trial, so as to ascertain what nature is likely to accomplish unaided, without waiting so long as to allow the mother to run unnecessary risk. Then, in case there is room for the introduction of forceps, they ought to be applied and delivery attempted by their means. If this is impossible, then delivery ought to be effected by cephalotripsy, craniotomy, or cæsarean section.

ARTICLE V.—*Case of Numerous Cutaneous Cysts scattered over the Body.* Reported by W. ALLAN JAMIESON, M.B., Berwick-on-Tweed.

PETER BUTLER, 45, labourer, a native of Ireland, but resident for many years in Berwick. Has always been healthy, the only illness he remembers having been an attack of sciatica some years ago, which yielded to hypodermic morphia. Was temperate, and personally more cleanly than many of his class. He is of medium height, with dark complexion, and thick dark hair. In July 1872 he first noticed a small lump, the size of a pea, under the skin, near the sternal end of the right clavicle. Soon after this he left his usual occupation, at some limestone quarries near Berwick, to engage in harvesting, and while thus employed, not feeling very well, he took a dose of Epsom salts. After (he says, in consequence of which) lumps, similar to the first, appeared on his body. As they did not inconvenience him, he paid little attention to them, till, finding his strength failing, he gave up work and consulted Dr Philip MacLagan, who, being interested in his case from its peculiarity, kindly asked me to see him along with him. At this time (8th February 1873) the tumours were chiefly small, varying from the size of small shot to nearly that of a pea. They were hard, and the skin was freely movable over each. One only, that near the clavicle, was as large as a small nut; the skin covering it was reddened, while fluctuation was distinctly perceptible on palpation. His general health seemed good. He then became an out-patient of the Berwick Dispensary, under Dr Brown's care, and I saw no more of him till the beginning of March last, when I succeeded Dr Brown at the Dispensary. The tumours had grown considerably, and Butler being anxious to have further advice, I gave him a note to Dr George W. Balfour, who carefully examined him, and ex-

pressed his opinion that they were cutaneous cysts. After his return from Edinburgh, Butler consented to allow me to excise one, so as to determine quite fully its nature. Unfortunately, in so doing the cyst wall gave way, owing to its delicacy and tolerably firm adhesion to the subcutaneous areolar tissue, in which it lay embedded. Its contents consisted of a turbid, brownish liquid, which on microscopic examination contained numerous epithelial cells. Butler gradually became weaker and more emaciated, the cysts grew more rapidly, and, from the absorption of the adipose tissue, stood out much more distinctly, so as to enable a photograph to be taken for Dr Balfour. In the beginning of June his state was as follows:—Nearly always confined to bed. Scattered over the body, chiefly the trunk, but to some small degree over the limbs, head, and face, are about two hundred and fifty tumours. There are about an equal number on the anterior and posterior surfaces, but none on the hands. Their size varies from that of a pea to a large nut. They lie immediately beneath the skin, which is freely movable over the smaller, to some degree attached to the larger. Over most, the colour of the skin is unaltered; over a few, it is reddened; while one or two of the largest near the sternum are greenish. Examined with a lens, no black point is discoverable. Fluctuation cannot be made out in the smaller—they seem hard, gristly, smooth bodies—but is quite distinct in the larger, conveying the impression of contents of some degree of consistency. In fact, in appearance they bear the closest resemblance to the common wen. The tumour which first appeared is now no longer there. Its contents must have been absorbed, as Butler assures me none ever burst externally. His tongue is red, and in parts denuded of epithelium; his appetite is defective; he is thirsty; while the bowels are confined, partly, no doubt, in consequence of opiates administered to procure rest. His temperature is 98.2° ; skin perspiring; pulse, 84, weak; heart and lungs healthy; urine, dark sherry colour, 1023, acid, contains neither albumen nor sugar, but some bile pigment; deposits, granular urate of ammonia, and some pus-cells.

Dr MacLagan reminds me of a symptom which I have omitted to mention—the extreme sensibility to external cold which Butler always exhibited when stripped for examination, even when standing in front of a warm fire at the dispensary. The results of treatment, which consisted in the administration of arsenic, etc., were *nil*. A species of spontaneous cure was apparently set up in more than one instance. By the reddening and inflammation of the skin covering the larger cysts (a form of natural counter-irritation, such as we strive to imitate—often successfully—by the use of iodine), absorption of the cyst contents was effected. This was undoubtedly the case with respect to the first cyst; and most probably also the same thing happened with regard to others. The cysts appear to have originated in the usual way—by obstruction of part or the whole of a sebaceous follicle, retention and accumulation of its con-





tents, hypertrophy of the enclosing membrane, and degeneration of the secretion. If the cysts arose from dilatation of a single sacculus of the racemose gland, an explanation would be given of the entire absence of the black point, as also of the absence of any tendency to open externally.

The photograph from which the plate has been engraved was taken in May by Mr R. Totherick, of Berwick. It represents very accurately the present appearance of the cysts.

ARTICLE VI.—*The Typhoid Condition.* By J. MILNER FOTHERGILL, M.D., M.R.C.P., London.

“No blunder is more common than to misconstrue into typhoid fever a typhoid condition of the system.”—(Da Costa.)

This distinct expression of opinion on the part of one so well and so favourably known in relation to medical diagnosis, will form an excuse at least for the consideration here of that condition denominated typhoid. The condition is one with which all are only too familiar. Still, our positive knowledge of it is quite disproportioned to its importance. As to how it is originated even we are in doubt; indeed, what we know of it consists rather in what we have good reason to regard as probable than in what we can actually demonstrate. It is generally regarded as being due to an increased metamorphosis of blood and tissues. It is therefore a very common and almost, indeed, necessary result of continued fever. The very name suggests this. A typhoid condition is one resembling the condition of typhus fever, where this state almost invariably occurs.

Previous, however, to going into the subject of the typhoid condition, it may not be undesirable to consider briefly the subject of tissue-waste or histolysis. We now know that the temperature of the body is the result of combustion within it. This combustion is maintained by the oxidation of hydro-carbons chiefly, though not entirely, for we have in urea and its allies the nitrogenized waste represented: they are really the ashes or cinders, the non-combustible or non-oxygenizable products of histolysis. There is every reason to suppose that the temperature is maintained by the hydro-carbons, in the form of lactic acid in union with a soda base (Headland), and that this combustion goes on chiefly, if not entirely, in the muscles. The body maintains its temperature almost entirely from the hydro-carbons assimilated from the food, the excess being stored as fat, which is deposited in the connective tissue, which, when so modified, is called adipose tissue; and there remains ready for future uses. When the amount of hydro-carbons requisite for maintaining the temperature are not supplied by food, as in starvation or in Bantingism, these stores are drawn upon, and emaciation by loss of fat results. It would appear that as the blood

deposits fat when the wants of the system are more than supplied, so it reabsorbs it when required. Thus, hibernating animals live during winter on the fat accumulated in summer and autumn. (In all probability circumstances favour this by the animal stowing itself away where its heat is not rapidly radiated off.) Carpenter states that in starvation the temperature is somewhat lower than the normal until the fat is consumed, and that then it rapidly falls. In the experiments of M. Chossat, it was found that, in producing death by starvation, those animals which lost most weight, in which the fat had been most abundant, were also those animals which lived the longest.¹

But while in starvation we have combustion of fat at a normal, or very little lower than normal temperature, in fever we have a very different condition. "It is chiefly the nitrogenous tissues of the body that are consumed during fever. Although there is a decrease in the amount of fat, there is no reason to believe that the rate of its diminution is greater than is accounted for by the state of inanition."—(Murchison.)

While we have in starvation a combustion in the muscles of the stored-up materials, with a temperature not above the normal, in fever, where the temperature is distinctly increased, we have a melting down of the nitrogenized tissues themselves. It is highly important that this be borne in mind in the consideration of the typhoid condition. The changes in combustion entailed by an increase in the temperature, and the consequences thereof, are very important, for it is upon them, in all probability, that the typhoid condition depends. "The urea or carbonate of ammonia, with other products of tissue-metamorphosis circulating in the blood, give rise to symptoms of uræmic poisoning (typhoid symptoms)." Such is the statement of Murchison ten years ago. Let us see how far this is substantiated by more recent observation. How far it is probably true we shall be better able to judge after reviewing the following considerations:—

1. The association of high temperature with the muscles.
2. The wasting of muscles in fever.
3. The sense of prostration (*debilitas febrilis*).
4. The excess of urea.
5. The condition of the muscular structures before and after death.

To take them in the order in which they stand, we have:

1. *The Association of High Temperature with the Muscles.*—This admits of being regarded from two points of view, one in connexion with the temperature of tetanus, and the other in the heat of the muscles after death in fever.

In tetanus, where the muscles are kept in violent and prolonged contraction, very high temperatures have been attained. The highest temperature on record is 112·55°, found by Wunderlich in

¹ Principles of Human Physiology, sec. 75.

² A Treatise on the Continued Fevers of Great Britain, 1862.

a case of tetanus. The high temperature of tetanus is not confined to life, but is found for some time after death. Thus, C. B. Radcliffe found the temperature in a case of tetanus to be no less than 111.8° one hundred minutes after death, and, six hours after, to be still 106.25° . In explaining it, he says:—"It seems as if one condition of this rise of temperature might be the removal of some cerebral regulating power; and beyond this it is difficult to see further, except it be that this paralysis, reaching to the vaso-motor nerves, allows the minute vessels to dilate and receive more blood, and that this state of congeation, even though the blood be stagnant, as it is after death, may lead to increased molecular changes, of which the additional degree of heat is the effect."¹ A still stronger evidence of the association of heat with muscles is supplied by Dr Bennett Dowler of New Orleans, who found that, on taking the temperature of various parts of the body of a man who had died of yellow fever, the highest temperature was furnished by an incision into the muscles of the thigh, which gave a temperature of 113° fifteen minutes after death; an hour and forty minutes after death, the temperature of the heart and thigh were alike 109° ; and that, three hours after removal of all the viscera, a new incision into the thigh gave a temperature of 110° . This places beyond all doubt the association of high temperature with the muscles, and not with the viscera, as was once held.

2. *The Wasting of the Muscles in Fever.*—Whether the muscular wasting in fever is the result of an increased temperature, or the increased temperature melts down the muscular structures, may not be positively affirmed, but there exists no doubt as to the fact. After a severe pyretic affection, the muscles of the limbs are often wasted to an extent quite surprising when compared to the adipose layers. The lax and shrunken hand betokens the diminution in bulk of the interossei and other muscles. In hectic fever, the muscles seem sometimes to have almost entirely disappeared ere death, the patient being, as it is called, "only skin and bone."

3. *The Sense of Prostration (Debilitas Febrilis).*—There is no more common concomitant of fever than a sense of muscular prostration; and when the typhoid condition is pronounced, the patient is found lying on his back in the middle of the bed—no matter how often he is lifted on to his pillow, he quickly slips down into the middle of the bed, and into his wonted attitude. The muscular prostration is then complete; but one of the earliest symptoms is muscular weariness. Nor can we wonder at this, when we find how largely increased are the products of muscular waste found to be in these pyretic conditions. Professor Samuel Haughton, F.R.S., in a paper entitled "The Relation of Food to Work done by the Body," taking the amount of urea as the evidence of work done, actually done, by the muscular combustion, says, in speaking of typhus fever, "Your patient lies for nine or ten days supine, fast-

¹ Dynamics of Nerve and Muscle.

ing, sub-delirious—the picture of weakness and helplessness; and yet this unhappy sufferer actually performs, day by day, an amount of work that might well be envied by the strongest labourer in the land.” He regards the animal heat given out in typhus to stand to that of health as 45 to 40. He goes on to say, “If you could place your fever patient at the bottom of a mine twice the depth of the deepest mine in Cornwall, and compel the wretched sufferer to climb its ladders into open air, you would subject him to less torture, from muscular exertion, than that which he undergoes at the hand of nature, as he lies before you, helpless, tossing, and delirious on his fever couch.” After this can we feel surprised at the terrible prostration of the later stages of fever, or at the muscular weariness which tells of the tissue-changes? Can we wonder, either, at some of the violent efforts of delirium occasionally manifested in the earlier stages, which are merely the outcomes of muscular combustion—other forms of force than heat?

4. *The Excess of Urea.*—After pyretic conditions we have increased quantities of urea, etc., in the urine. It might appear more correct to have said “during” than “after,” but “after” is probably more correct in relation to the causation. “There is found, too, on the whole, a direct relation between the temperature and the amount of urea,” says Murchison; and again he says, “According to Brattler there is a close correspondence between the amount of urea and the temperature, the greater the amount of urea the greater the temperature.” Not only is urea found to be increased in typhoid fever, but uric acid, and the other products of histolysis, are also found in excess. This increase in the waste products, or, as denominated above, the ashes of histolysis, is very considerable. Vogel and Warnecke found 1065 grains of urea excreted in 24 hours in a case of typhoid fever, the normal amount of urea being about 400 grains. In a case of pyæmia, 1235 grains of urea were excreted in one day. The amount of uric acid excreted daily is about 8 grains, but in a case of fever, Handfield Jones has found it as high as 23·76 grains. Dr Parkes has given great attention to the relation of urine to temperature, and in a case of typhus he examined the urine for eighteen days consecutively, and found the proportion on the average to have been 530 grains as compared to a normal of 400 grains. The increase was found every day. The relation of urine solids to temperature cannot yet be regarded as finally settled, but such indications as are warranted by the present state of science will be referred to again shortly.

5. *The Condition of the Muscular Structures before and after Death.*—As might fairly be deduced from the foregoing considerations, the muscular structures after death are much affected in pyretic affections. Laennec long ago pointed out the changes that are undergone by the voluntary muscles in typhus, and his observations have been followed out by others, notably Zenker, on the muscles after typhoid; and his observations, in turn, have been

followed out by Buchanan in typhus. The changes are not so marked if death have taken place early in the fever, but after the third week, or later, the degeneration is well marked. "There are two forms of degeneration, the granular and the waxy. The granular form consists in the deposition of minute highly refracting granules in the contractile tissue, giving to the fibres a dark appearance by transmitted light, and obscuring the striæ. This muscular deposit is not wholly composed of fat. The degenerated fibres are very friable. The waxy form consists in the transformation of the sarcous tissue into a homogeneous colourless mass, glittering like wax, and causing a complete obliteration of the striæ and nuclei of the fibres, the sarcolemma remaining intact. The waxy cylinders thus formed crack up into numerous fragments, which crumble down into a finely granular detritus, and this is gradually absorbed."—(J. Harley.) Rindfleisch gives two engravings of this myositis typhosa, which show these changes under the microscope. These changes in the muscular structure of the heart in relapsing fever, where the temperature is high (105.8° to 107.6°), form one of the principal dangers in this not very fatal form of fever; and sudden death from effort during convalescence, as in perambulating the ward, has been known, from cessation of the heart, enfeebled by this degenerative change.

Nor are these changes in the muscular structure either of a post-mortem character or confined to fatal cases. By means of the modern invention of "harpooning" muscles, portions of voluntary muscle have been procured for the purpose of microscopic observation, and the changes just mentioned have been found to exist during the lifetime of the patient.

From these different considerations we may fairly assume that in pyretic affections, and especially in continued fevers, we have such chemical interchanges going on that the muscular structures of the body are seriously affected; that, in fact, we have changes going on which can be visibly demonstrated, and that these changes are of the character of tissue-decay, the fatty portions of the albuminous tissues being left; and along with this we have a distinct increase in nitrogenized waste, as seen in the urinary solids. These changes are accompanied by great and increasing muscular prostration during the febrile state, and by a great diminution in the bulk of the muscles after the febrile condition has passed into convalescence. The conclusion to which these facts point is inevitable, or nearly so. Further testimony can be brought to associate these changes even more pointedly with excess of urea and the typhoid condition. Buchanan (article "Typhus Fever," in Reynolds's *System of Medicine*) says, "In cases of typhus terminating fatally through the intensity of the disease itself, prostration, subsultus, and carphology increase, and low delirium passes into complete coma. The pulse and temperature continue to rise." In speaking of the daily excretion of urea, he says, "The increase is found at the earlier

day at which the urine has been examined. In three fatal cases it did not diminish in quantity during the time that the urine could be procured." And further on again, "Convulsions in typhus are almost always associated with albuminous urine, and in a few cases, where this has been investigated, with urea in the blood." In the article, "Typhoid Fever," Harley says, "The quantity of urea and uric acid excreted, appears to be proportionate to the degree of fever; when the pyrexia is at its height, the quantity of these constituents excreted in twenty-four hours is usually doubled, sometimes trebled." We will now see how far the experience of Murchison has led him to similar conclusions. "As in other febrile conditions, the increased formation of urea, notwithstanding the diminished supply of food, is evidently the result of an exaggerated disintegration of the muscular and other nitrogenous tissues. As long as the urea continues to be eliminated by the kidneys, its effects are comparatively trifling; but if the quantity be excessive, and still more if, from any morbid condition of the kidneys, either antecedent to or resulting from the febrile attack, its elimination be interfered with, it accumulates in the blood, and gives rise to uræmic (typhoid) symptoms. If the urine be completely suppressed, as sometimes happens, death speedily ensues, under symptoms of coma, and sometimes with uræmic convulsions; but if the suppression be less complete, it may still give rise to delirium, stupor, and coma." He then gives cases from Michael Taylor, Christison, and Frerichs, where, after death from typhus with marked cerebral symptoms, the kidneys being found healthy, urea was found in the blood, and that too in considerable quantities, during life. These cases he corroborates from his own experience; saying, in conclusion, "The observations which have been made in relapsing and pythogenic fevers also support the opinion that the head symptoms in typhus are due, not to inflammation, as was once believed, nor to the presence of the original fever poison in the blood, but to the circulation through the brain of urea, carbonate of ammonia, or other products of disintegrated tissue."—(Pp. 146, 147.)

There seems little doubt but that in febrile affections there results such waste of tissue as produces a typhoid condition. But is this typhoid condition a consequence of the fever, as a specific fever, or is it due simply to the rise of temperature? For this is really the question at issue. If the typhoid or uræmic condition is the result of tissue-waste, as we have just seen it stated to be, what we have to do is to connect excessive nitrogenous waste with non-specific high temperatures, and then the reason why we have a typhoid condition occurring more or less frequently in such affections is not far to seek.

Unruh¹ made an examination of the urine, as to the excretion of nitrogen, in twenty-eight cases of pyretic affections; nine were cases of pneumonia, four of typhus, ten of enteric fever, two of relapsing

¹ Sydenham Society's Year-Book, 1869-70.

fever, two of syphilis, two of acute rheumatism, one of trichinosis, and the remaining six of local inflammations. On the average of the twenty-eight febrile cases, the excretion of nitrogen was half as much again as in health; in some of the cases, it was twice as much.

Naunyn¹ made experiments on dogs, in confirmation of the views of Traube, Jackman, and Vogel, as to the increased secretion of urea in fever in men. His first set of experiments having proved this, he proceeded to inquire whether the increased secretion of urea is primary, and the cause of the increased temperature, or whether the latter is the cause of the former, and from fresh experiments he concluded that the increased secretion of urea (and other compounds containing nitrogen) occurred in consequence of the increase of temperature.² Thudicum states that in all febrile conditions, however caused, the following changes take place:—(1) Temperature, and (2) disintegration of albuminous substances are increased, etc.³ If then we have increased nitrogenized tissue-waste in these conditions—and it would appear that there is such tissue-waste in these conditions, as Naunyn's experiments very conclusively tell, even when that febrile condition is produced under the simplest conceivable circumstances—it follows that typhoid symptoms may also then occur.

It does not necessarily follow that they must occur.

Great waste may be met by great renal activity, as in Vogel's case mentioned before, and consequently there may be no accumulation of the products of tissue-metamorphosis in the blood. It is very obvious that any pre-existing condition of renal inefficiency will conduce to accumulation of these waste products in the blood, under conditions where the increase is but slight; and that, on the other hand, renal activity, which almost implies integrity, will prevent the occurrence of a typhoid state in cases where the temperature and the tissue-changes are both much increased.

Accumulation may alike arise from non-elimination as from excessive production. This is a conclusion about which we may feel pretty sure after the foregoing considerations. But let us see how the matter actually stands, how far such conclusions are corroborated by clinical experience. Murchison, in speaking of prognosis in typhus, says: "The gouty diathesis, from its being so often associated with disease of the kidneys, is a very serious complication. I have never known a gouty person attacked with typhus recover." He has also found that suppression of urine, or a fall in its specific gravity, are the precursors of coma and convulsions, while a highly

¹ Sydenham Society's Year-Book, 1869-70.

² Naunyn found that when heat was retained within the body of a dog by placing it in a warm atmosphere saturated with moisture, the temperature in the rectum rose $7\frac{1}{2}^{\circ}$, and the urea rose in the next 24 hours from 6·7 to 9 grammes.—*Journal of Anatomy and Physiology*, May 1871, p. 408.

³ Sydenham Society's Year-Book, 1869-70.

febrile condition may pass away without danger when the urinary solids are present in great excess. The actual presence of the waste products of tissue-change are but one factor in the production of the typhoid state—so important a factor, however, that a temperature of 107.6° in typhus is said to be incompatible with recovery. The changes caused by a temperature higher than this are followed by death; but even a lower temperature may occasion a condition resulting in death under certain other conditions. "Although the occurrence of cerebral symptoms, and of the typhoid state in particular, be mainly due to the retention in the blood of the products of tissue-metamorphosis, other circumstances probably contribute to their development, such as the abnormal or defective nutrition of the central organs of the nervous system, and the non-aëration of the blood consequent on pulmonary complications."—(Murchison.) The accumulation of waste products is the important, indeed the essential, factor in the production of a typhoid or uræmic condition.

We have seen that an increase in tissue-waste is a consequence of all febrile conditions, however produced. It follows then that this typhoid state may be found in the progress of such affections as entail a heightened temperature, especially if the increase of temperature be maintained for several days. Such is the experience of all who have given attention to the subject. Were such recognition more widely extended, we would not so frequently meet, hear, and read of the blunder which Da Costa says is so common. It is not the mere avoidance of the blunder, and thus the saving of our self-pride. The true understanding of the conditions under which a typhoid state may arise would not only have spared many a mistake, long and bitterly regretted, but would prevent much that is painful, if not for the patient, at least for his friends; would prevent many steps from being taken which have been taken; and would prevent many an outlay which has been irritating to the patient's family, and been the cause of no little obloquy being thrown on the profession. It is always unfortunate when patients suffering from other affections are sent into a fever hospital as suffering from fever. First, there is the risk of such patient dying from fever caught directly through a diagnostic error, his death actually resulting immediately from the mistake made; and, second, there is the reflection upon the profession which naturally follows, to say nothing of the pain to the medical man himself in such a case. Not that such mistake can always be avoided, for even Murchison himself writes: "It is not surprising that uræmia from renal disease is apt to be mistaken for typhus; indeed, it is sometimes impossible to distinguish the two diseases." That conditions so allied in their origin should closely simulate each other in their symptoms, can be no matter for surprise. Such being the case, it behoves us to be all the more upon our guard lest we ourselves fall into such error.

Other conditions, as low pneumonia, chiefly of both apices,

pyæmic or septic states, hectic fever, dysentery, erysipelas, meningitis, acute tuberculosis, prolonged local inflammations, etc., as well as conditions depending on renal disease, are found with this typhoid state following or accompanying them. In fact, all conditions of high temperature, whether due to specific fever, or to what are called for distinction exanthemata, or to idiopathic conditions, may result in these typhoid symptoms, especially if the increase of temperature be marked. It has been found, indeed, that a temperature of 108.5° is incompatible with life. A rise, and a rapid rise, often precedes death; but whether the agony is the cause or the result of the high temperature is not yet finally settled; the evidence, however, preponderates in favour of the agony being the result, the rise of temperature often lasting for some time after death.

And now as to the outward indications of the condition whose pathology we have been reviewing at length. In the progress of a fever "it is announced by the decline of the previous more acute symptoms; by the pulse becoming more rapid and soft; the tongue dry and brown, tremulous, and protruded with difficulty; by the incrustation of the teeth with sordes; by the increasing intellectual disorder, indicated by the more constant low muttering delirium, and the greater insensibility and deafness; and by the condition of the muscular system, evinced by muscular tremor and subsultus tendinum, and in some cases irregularity or intermission of the pulse, by the patient lying sunk on his back, or sliding to the foot of the bed, the muscles being unable to support the body even in the horizontal posture."—(Tweedie.) To this graphic sketch there is little to add that is important. When the condition is uræmic rather than typhoid, there is little difference, except that the active brain symptoms preceding coma and convulsions are often wanting. This is probably due to the difference in temperature, which is much increased in the typhoid state, and to some extent to the specific nature of acute zymotic affections; the difference, however, being but slight in acute uræmic conditions, as in the case of acute suppuration of the kidneys given by Da Costa,¹ which was almost undistinguishable from typhoid fever. In uræmia supervening upon chronic renal disease, the delirium is apt to be wanting, at least as a marked symptom, though even here the temperature is affected. Rosenstein² says: "The temperature, as I have found, is considerably increased (beträchtlich erhöht) during uræmic attacks, more during the convulsions, but also during coma." In uræmia we find stupor rather than acute delirium, soon passing into coma and convulsions. With such a scene as described above all are familiar, whether occurring during a severe specific fever, or closing the scene in what Bence Jones calls the peroxidizing exacerbations of Bright's disease. Whenever occurring and where, it is ever a serious condition, fraught with grave danger, and indicating, too, that the danger is near at hand. It marks periods of peril from which the patient may or

¹ Medical Diagnosis, p. 471.

² Nieren-Krankheiten, s. 152.

may not emerge, but which he is certainly entering. The clouded consciousness indicates that a dark valley of shadow is being approached, and the only light to guide our prognosis is a dim and feeble ray thrown out by probability. The muscular prostration indicates the gravity of the tissue-changes, and the indications so suggested are corroborated by the thermometer and corrected by examination of the urine. When occurring in the course of acute specific diseases, and perhaps in some of the exacerbations of Bright's disease, there is somewhat less of peril in this condition than when it occurs under other circumstances; but at the best we must never forget that, when this typhoid condition is developed, the patient indeed may be said to be "in the valley of the shadow of death." It marks a disturbance of the equilibrium of the body, either of the amount of tissue-waste or of inefficient power of elimination. When occurring in the course of acute pyretic affections, it indicates such a condition of distinct histolysis as is unmistakable—the tissues are melting down by union with oxygen, and a process inaugurated which is intimately allied to post-mortem putrefaction. The balance of the body is rudely shaken, and the limits gravely approached beyond which restoration to health is impossible; as we see a balance so far overthrown that the few remaining oscillations but delay the inevitable end, and do not admit of the balance being recovered. For, as we find there are limits of temperature which permit of what we term health, any oscillation beyond either its minimum 97° , or its maximum 101.5° , constituting disease, so we find an outer limit within which the oscillations of disease still permit of recovery; but if these limits are exceeded (92° and 108.5°), death must result. But there are other indications which point to the probable result as well as the temperature, such as the persistence of the posture on the back, indicating great muscular prostration; the starting of the tendons, due, of course, to contractions of the muscles in connexion with them, and differing but in degree from general convulsions; the inertness of the sphincters; the disturbance of vision indicated by the picking of the bedclothes; the glazing of the eye; the graver indications even of disturbance of hearing, as the imaginary ringing of bells; the retraction of the brown chapped tongue; the sordes on the teeth, accompanied by the formation of crusts on the lips; by the hissing respiration, so characteristic of this condition, and contrasting with the stertorous breathing of apoplexy; and very frequently by a urinous odour of breath due to ammonia, which forms crystals readily on a glass slide moistened with muriatic acid. Very often, too, in this condition do we find "the phenomenon of Cheyne," the ebbs and flows of respiration, a few deepening breaths marking the flow, and then a gradual shallowing of the respiration to one or two almost indistinguishable inspirations marking the ebb, and then again a gradual deepening until the flow is once more attained. Fifteen respirations, or so, usually complete the

circle. This phenomenon is associated with various grave conditions.

The typhoid state is almost certainly followed by death when it has come on gradually and lasted for weeks, as I have known uræmia do in at least two marked cases. Somewhat of change occurred, as a lightening of the symptoms, and then again a deepening of them, each oscillation marking a graver stage, until death terminated the scene.

When we remember the marked characteristics of this typhoid condition, we can scarcely wonder that, up to a comparatively recent period, the oncome of such a condition was described as the original disease "turning to typhus."

The older physicians, whose recognition of a disease was much more largely founded on mere naked eye changes and appearances than is now the case, and who were not so well acquainted as we fortunately are with both pathology and etiology, really thought that the affection under which the patient laboured was veritably converted into typhus fever. Their idea of typhus was much more comprehensive than ours is, and admitted both enteric or typhoid fever, and what are now regarded as typhoid conditions. Nor can we wonder, such being the case, that when they saw symptoms so allied to those of advanced typhus, and which they regarded as pathognomonic of it, symptoms not only allied to it in appearance, but also in gravity and danger to life, they thought indeed that typhus had supervened. What unfortunate results ensued from this, especially as to fear of infection in the alarmed relatives and attendants, it is needless to say. It is not likely that such error is common now; still there is reason to fear that old impressions linger long after they are supposed to be extinct, and it is possible that there may still remain here and there vestiges of this ancient and erroneous pathology. Should the considerations given here have the effect of rousing some physician connected with a large hospital to institute careful comparisons betwixt the typhoid condition in specific fevers and an allied state in non-specific affections, betwixt these and the condition more distinctly known as uræmia, not only as to symptoms, but as to temperature and the amount of urea, or the suppression of urine—indeed to clearing up the mystery still hanging over this important and interesting subject, and among other things annihilating utterly the old impressions of a disease changing to typhus, it would indeed be well; and the work would form a decided addition to our knowledge.

As to how such elevation of temperature as leads to the institution of increased histolysis is primarily originated, it is not yet possible to speak definitely. The existence of a centre in the cerebro-spinal system which regulates temperature has been hypothecated by Fischer, much as the vaso-motor centre of Traube and Ludwig governs vascular changes; but such temperature centre is yet merely an hypothesis, resulting, however, from both experimentation and

observation. Murchison thinks that "the nervous system (and particularly the sympathetic and vagus) is paralyzed." Virchow puts it thus: "Fever consists essentially in elevation of temperature, which must arise in an increased tissue-change, and have its immediate origin in alterations of the nervous system." About the first half, there is little if any doubt remaining; as to the second half, the conclusion is not so simple. That the nervous system is affected in specific fevers, there is no doubt, but the reverse seems almost equally established. Blandford ("On Insanity") writes: "At one end of my list are those suffering from acute delirium—acute delirious mania—which runs a rapid course to death or amelioration in a week or fortnight. Here we see, for the time, entire sleeplessness, incessant action of brain and body, with the evolution of great heat, speedy emaciation, a quick pulse, tongue coated and soon becoming brown, symptoms pointing to an excessive decomposition of every tissue, and a general excess of brain action, which, if it does not cease by a certain time, leads to death without apparent morbid change." Here we have a condition closely allied to an acute febrile affection; indeed, in these concatenated compound conditions it is not easy to say which change stands first. Does the brain disturbance affect the regulation of temperature? or does the increased evolution of heat stand to the brain disturbance in the relation of cause, and not effect? The question is a fairly legitimate one, and in time an answer of a positive character may be forthcoming.

While the typhoid state is ever a serious one, the seriousness of each case must rest on the peculiar circumstances of such individual case, which must be well weighed, and carefully appraised. No rule-of-thumb can possibly be laid down. But if the prognosis must remain a moot question, are there any indications for treatment to be derived from the consideration of the pathology of the typhoid condition? Most undoubtedly there are, and modern medicine has largely availed itself thereof!

It is obvious that agents which tend to lower temperature are here indicated. Such agents the Germans have found in digitalis, quinine, and alcohol. The observations of Ackerman told him that when the temperature fell the blood-pressure rose; and when the blood-pressure fell the temperature rose. That agents which act on the vaso-motor centre and produce contraction of the two muscular ends of the circulation, the central heart and peripheral arterioles, should lower temperature, is only what might fairly be expected, if paralysis of the sympathetic is the immediate cause of fever. The direct lowering of temperature in febrile conditions strikes at once at the changes which result from an elevated temperature. But it may happen, and probably often does, that the remedial agents are not absorbed well in this condition; or there may be persistent vomiting, or other objection to the use of internal remedies; in such case the excessive heat may be reduced by the external application of cold, or the measures may be combined. The latter treatment is rapidly

gaining ground, and is, after all, but the logical outcome of the doctrines of Sydenham.

The effect of a cold bath on a fever-stricken patient is to restore his consciousness and relieve the sense of prostration,—in fact, to relieve him of the consequences of his high temperature, by aiding in the radiation of his superfluous heat. For, as we saw by Naunyn's experiments, the effect of increased heat is waste of the nitrogenous tissues. Popular impressions will, it is to be feared, long obstruct the general adoption of this direct means of lowering temperature, but the dispelling of them will not be accomplished by passive reasoning, but rather by active measures, leading to conviction by demonstration. While writing this paper I came across a case of typhoid fever with a temperature of 106.8° , successfully treated by cold baths in Addenbrooke's Hospital, Cambridge. Dr Bradbury says, "The immediate change produced by the baths upon the patient was remarkable. Her intellect became much clearer whilst she was in the baths, and she fell asleep after them. The frequency of the pulse was much reduced."¹

While one branch of therapeutics is the attempting to check the excessive formation of these histolytic products, another is the trying to remove them. To attain this it is necessary to maintain the action of the kidneys, and consequently preparations of potash in combination with vegetable acids are deservedly in repute in the treatment of pyretic conditions. The bitartrate of potash in the form of *potus imperialis* forms an agreeable beverage as well as being no mean therapeutic agent. Position, too, can be utilized, as seen in the plan of rolling over the patient from his position on the back, first to one side and then the other. This relieves the congestion of the kidneys alternately, and is found to be practically useful. Another therapeutic measure is to take advantage of the capacity of one emunctory to help another. Thus, free purgation will effect more or less what the kidneys cannot do unaided, and the catharsis so induced will relieve the general condition. Indeed, in *uræmia* spontaneous catharsis is very common. Under ordinary circumstances the action of the skin supplements the work done by the kidneys, but in pyretic conditions free perspiration is not so easily induced. When practicable, diaphoresis may be found useful. Looked at from a rational point of view, the administration of food and stimulants must also affect this condition; and it would not be discreet to administer anything which would increase the histolytic products already in excess. Consequently the use of the fashionable extracts of meat is not without its objections. These extracts consist of derivatives of flesh which are really in the commencement of histolysis. They are already too far advanced on their retrograde career to be regarded as food, being no longer in a condition where they may be utilized in construction. They are stimulants, and correspond rather to vegetable principles, as *theine*, than to food in its true sense.

¹ British Medical Journal, 14th December 1872.

In conclusion, the writer apologizes to the reader who may have followed him so far, if the material seem defective, or but indifferently arranged. That the evidence is not yet complete, and that there is much yet to be learnt about the typhoid condition, is unfortunately but too true. If the reader finds that the perusal of this paper has tended, however little, to improve his own impressions as to the typhoid state, the writer's object will be largely obtained; if the perusal should lead to some systematic observations which will still further illumine this somewhat obscure chapter in medicine, it will indeed be gratifying.

ARTICLE VII.—*Two Cases of Intussusception in Children.* By
J. O. AFFLECK, M.D.

(Read before the Medico-Chirurgical Society of Edinburgh, 4th June.)

ALTHOUGH liable to occur at any period of life, intussusception is an accident remarkable for its relative frequency in young children, more especially in those under the age of one year. The statistics of the disease show that it occurs more frequently in the first year of life than in the whole of the succeeding twelve years. Its occurrence in early infancy is marked by certain well-ascertained peculiarities which it is of importance to bear in mind, both as distinguishing it from the same affection at later periods of life, and as influencing our diagnosis, prognosis, and treatment. The following facts appear to have been satisfactorily established by numerous observations, and by statistical record of cases:—

1. That the seat of intussusception in young infants is almost always some portion of the large intestine, usually the neighbourhood of the ileo-cecal valve.
2. That while a cure by the spontaneous reduction and replacement of the invaginated bowel seems occasionally to be effected, yet that that process of spontaneous cure by sloughing of the bowel and subsequent cicatrization, which is sometimes observed in adults and in children beyond one year, appears almost never to take place in young infants.²

¹ Statistical Researches relative to Intussusception in Children, by Dr J. Lewis Smith, American Journal of the Medical Sciences, January 1862. *Traité des Maladies des Enfants*, par MM. Barthéz and Rilliet, 2d ed., tome i. p. 802. Drs Meigs and Pepper on the Diseases of Children, 4th ed., 1870, p. 419. Dr West on the Diseases of Infancy and Childhood, 5th ed., 1865, p. 616.

² In the *Lancet* of 17th May 1873, there is a short notice of a remarkable case of intussusception in an infant of six months old, occurring in the outpatient department of the Victoria Children's Hospital, under the care of Dr Evan. Sloughing and discharge of a portion of the invaginated bowel took place, but death followed rapidly. The history of the case shows it to have been one of a very exceptional character.

3. That in them the morbid phenomena following an intussusception advance with great rapidity and fatality, and that treatment to be effectual, must be prompt and active.

These remarks appear to be necessary before proceeding to narrate the particulars of two cases of this formidable though happily rare malady, which, by a singular experience, came under my observation within the short space of nine months.

CASE 1.—James H., a strong and healthy-looking infant of seven months old, was seized, after awaking out of a long sleep, on the evening of Wednesday, 7th June 1872, with vomiting and symptoms of abdominal pain. The child, who was nursed entirely at the breast, had all along enjoyed fair health; the only exception to this being the occasional occurrence of diarrhœa. I was requested to see the infant on the evening mentioned, but it was late before I could attend, and by that time the symptoms had somewhat abated and the patient was quiet. There was nothing alarming in his appearance. On the following day (8th June) the infant seemed still easier. There had been a slight evacuation of the bowels, but there was still occasional vomiting, to allay which some powders containing bismuth were prescribed. Next day (9th June) the child appeared worse, vomiting severely, and apparently suffering much abdominal pain. Hot fomentations to the abdomen were ordered, and a small quantity of castor-oil, as there had been no further evacuation of the bowels. On seeing the infant the following day, Saturday (10th June), I was informed that some bloody matter had been discharged *per anum*, but that there had been no fecal evacuation. During all this time the infant took the breast, but the milk was almost always quickly vomited. The occurrence of the bloody discharge, taken in connexion with the previous symptoms, left little doubt in my mind that this was a case of intussusception, and I proceeded to treat it by means likely to replace the bowel. I administered large enemata of warm water, but these were expelled so quickly and violently that I had soon to desist. The finger introduced into the rectum could detect no obstruction, but there was great downward straining. There was no hernia nor tumour to be made out on external examination. I then commenced to inflate the bowel, using at first the ordinary indiarubber enema syringe, but I soon found that this was far too feeble to distend the intestine, and so, detaching the long tube of the syringe and fixing it to the nozzle of a pair of bellows, I proceeded to blow in air cautiously for two or three minutes. The large intestine became well inflated, but after repeated trials both with air and water I was ultimately constrained to desist from further attempts of this nature. My friend and neighbour, Dr Husband, kindly saw the case and assisted me in my attempts at inflation. By this time the child was reduced to a state of extreme prostration, and the vomited matter had a fecal odour. As a last resource, the administration of a small quantity of quicksilver was tried, but

without effect. The vomiting continued, and the infant sank and died on the afternoon of Sunday the 11th June. Some hours before death a small quantity of feculent matter came from the bowels.

On post-mortem examination (24 hours after death), in which I had the assistance of Dr Husband and Mr Oswald Wood, there was found, deep in the right iliac fossa, an intussusception of about three inches of the lower end of the ileum into the cecum. The bowel was so tightly constricted and so fixed in its position by inflammatory adhesions that it was with difficulty that it could be reduced. The bowel at the seat of stricture was intensely congested, and had brownish black patches over its surface. Below the seat of stricture the large intestine was pale and empty, while above, the small intestines were much distended. There was a small quantity of fluid in the abdominal cavity. There was no other morbid appearance.

CASE 2.—On the forenoon of Wednesday, 27th March 1873, I was requested to see M. M., aged five months, who was stated to have turned suddenly and alarmingly ill. The infant, who had all along been in good health and was nursed at the breast, had for some days previously slight diarrhoea, but not of such amount as to require treatment. Between 9 and 10 on the morning mentioned, the child had a copious evacuation of the bowels of normal character. She was then lively and cheerful, but in a few minutes thereafter she suddenly uttered a loud cry of pain, and then vomited much and violently. After the stomach was emptied, the child retched severely, and this retching continued at intervals of a few minutes with great violence.

As the symptoms were unusual and alarming, an urgent message was sent to me. On seeing the child, I was struck with her pale, exhausted look. Her pulse was small and rapid, and her surface cold. She appeared as if in a state of shock. While I was examining her, sickness came on, accompanied with violent retching, which lasted for about three minutes, when the little patient sank back exhausted. The evidence of great abdominal pain, together with the suddenness and violence of the attack, excited my suspicion of an obstruction of the bowels; but without expressing my fears, and in the hope that the symptoms might possibly arise from some temporary gastric disturbance, I prescribed bismuth and magnesia, with the view of allaying the vomiting. I again saw the patient about 9 p.m. Her mother stated that all day the infant had had persistent sickness and retching, and great pain. She sucked the breast eagerly, but the milk was always quickly vomited, and even when no food had been given, the vomiting continued to recur. There had been no further fecal discharge from the bowels, but my attention was attracted to a quantity of blood and bloody mucus which had been discharged *per anum* several times in the course of the evening. The conclusion that an intussusception had occurred could no longer be avoided, and I proceeded at once to treat the

case under this impression. The finger could detect no obstruction in the rectum, but the severe straining efforts caused a little mucus tinged with blood, but without fecal smell, to come away. The abdomen was soft, and was throughout dull to percussion. There was no tumour nor hernia to be made out. Manipulation elicited pain. Enemata of warm water were administered, but they were expelled so rapidly and violently that it was vain to persevere with them. I then resolved to have recourse to inflation, in the manner already described, and I blew in air for two or three minutes. There was great resistance offered to the entrance of the air, and thinking the bellows too weak, I procured a stronger pair, and continued the inflation for a minute or two longer. The infant, who had submitted to the previous operations unmoved, suddenly gave a cry of pain, and almost immediately I felt the resistance yield and the air enter the bowel readily. The mother, who held the infant, and had her hands on its flanks, felt the same sensation of something giving way. I withdrew the tube for a little, and then inflated again, when the intestine became well distended with air, and the abdomen tympanitic. The infant was put to the breast. She drank eagerly, and no vomiting followed. She was subsequently placed in a warm bath, and a very small quantity of castor-oil administered. She fell asleep, and on awaking, about four hours after, it was found that the bowels had moved copiously, as they did several times afterwards in the course of the night. There was no more vomiting nor pain. On seeing the patient the following morning, it was difficult to believe her to be the same child. She was then perfectly well, and she has continued so ever since.

These two cases, happening at about the same period of life, afford a fair illustration of the fact, well known in the symptomatology of this disease, namely, that the phenomena of its occurrence are by no means uniform either as to time or degree; and that while there are undoubted marks by which the disease may be recognised, the chief symptoms are less characteristic of the intussusception as such, than of the subsequent obstruction or strangulation of the bowel. In the first case the initial symptoms, though distinct enough when read in the light of subsequent events, were not of themselves sufficient to indicate the occurrence of so serious an accident as intussusception. This is quite in keeping with what is very commonly observed in this disease. A portion of bowel becomes invaginated. Vomiting, abdominal pain, and other symptoms of disturbance, may arise, or they may for a time be entirely absent, or, at all events, so slight that no vigilance on the part of the physician could avail to recognise the grave nature of the case. It is only when constriction of the invaginated portion of gut, and consequent obstruction, have arisen, that urgent symptoms appear, and then, unfortunately, it too often happens that other morbid processes, such as peritonitis, with adhesions at the seat of stricture, have been set up, or, as is

so frequently the case in infants, that the strength of the patient has declined to a degree that excludes the hope of relief by remedial measures. This is what appears to have occurred in the first of the cases now under consideration. The lower portion of the ileum had slipped through the ileo-cecal valve and become invaginated in the cecum. Some symptoms, such as vomiting, immediately arose, but nearly two days had elapsed before these became sufficiently marked to denote an obstruction of the bowels. The probability is that complete strangulation did not take place till about this period. Although active efforts were adopted as soon as the mischief was recognised with the object of removing the obstruction, it is very doubtful, even if these had succeeded in reducing the intussusception, whether recovery could have followed, seeing that peritonitis had begun, and that the patient's strength was almost completely exhausted.

The second case differs from the first in the sudden development and rapid advance of the symptoms of intussusception. The recovery of the patient fortunately gave no opportunity of verifying the correctness of the diagnosis, but the history of the case, as already detailed, can scarcely admit of any doubt in this matter. Indeed, it was the unmistakable evidence afforded by the symptoms which drew attention to the real nature of the case, and suggested the employment of treatment in time to save the patient's life.

The invagination had in this case occurred either at the valve or at some point lower down in the large intestine. That the latter supposition is the correct one is rendered probable by the fact of the strong resistance which was offered to the entrance of the injections. However this may be, it can scarcely be doubted that strangulation of the gut was complete at an early period, and that had treatment by inflation been delayed, it would have failed in this case as in the former. As it was, the result of the treatment was manifest and most satisfactory; for not merely was there the distinct evidence of the sudden yielding of the obstruction, but there was the almost immediate cessation of the urgent symptoms, the restoration of the normal functions of the bowel, and the speedy recovery of health.

The causes rendering this disease more liable to occur in children have been much discussed. The limits of this paper forbid any lengthened consideration of them. Although intussusception does appear to happen often in the midst of perfect health, yet a reference to the recorded cases shows the frequency of some previous irritation of the bowels, and the great functional activity of the intestinal canal in infancy cannot be without its influence in predisposing to the occurrence of the accident. As explaining its almost constant seat in infants, viz., at or near the ileo-cecal valve, the comparatively circumscribed attachments of the cecum to the iliac fossa at this period of life, and the imperfect development of the muscular coats of that portion of the gut, have been mentioned by

several writers, particularly by M. Rilliet. Whatever value these alleged causes may possess in a pathological aspect, there can be no doubt of this, that intussusception occurring in early infancy is a most fatal malady.¹ Few recoveries are recorded as occurring among infants under the age of one year, thus differing in a striking manner from the same disease as affecting children beyond this age and adults, in whom the proportion of recoveries is considerable.² Nevertheless, treatment, if had recourse to in time, may succeed in the one case as well as in the other. And, indeed, the situation of the disease in infants—viz., the large intestine—brings it within reach of what is really the most valuable method of cure, that of inflation. This plan, which dates as far back as the time of Hippocrates, is as simple as it is efficacious. But, to succeed, it must be resorted to early. It is well known from actual experiment that it takes very little force to reduce a recently invaginated portion of intestine, for it is occasionally possible to accomplish this by manual pressure when the obstruction comes so low down as to be within reach of the finger. But should inflammation arise to produce adhesion of the invaginated portion of bowel, our efforts by injection either of water or air will prove abortive, and then there remains the desperate alternative of a surgical operation or speedy death. Into the important question of surgical interference in such cases I cannot now enter. Any interest which may attach to the cases narrated must plead my apology for the length to which this communication has been prolonged.

ARTICLE VIII.—*The Use of the Electro-Magnetic Current in the Second Stage of Labour.* By A. E. M'RAE, M.D., C.M., Penicuik.

(Read before the Obstetrical Society of Edinburgh, 11th June.)

FEW things have received more attention from practical men, than the aids to human parturition. Among the many appliances suggested, none have received less attention than galvanism. No doubt the failure to utilize its power has, by indiscriminate and undefined use, led to disappointment and consequent neglect. I desire to place the following three cases, which occurred in 1868 and 1869, on record, so that others, who may have been discouraged in its use, may again be induced to try it when a suitable case presents itself. I use the words electro-magnetic and electro-galvanic and galvanic as practically well-nigh synonymous.

It is well known that galvanism is a powerful agent in exciting

¹ Dr Lewis Smith states that he knows of no case of recovery under one year. Rilliet, however, mentions two; and Meigs and Pepper also refer to occasional recoveries by treatment at this early period.

² More than one-third, according to Smith.

muscular contraction. The uterus being a muscular body is susceptible of its influence. Some high authorities in gynæcological science, reasoning more from analogy than from actual experiments, have given their opinion against it. Later and more extended observations, however, have shown that galvanism, under certain conditions, has a powerful influence over uterine action. The following cases will, I think, in some measure support the theory:—

CASE 1.—Mrs S., aged 28, a well-made woman of about 5 feet 6 inches in height, was in labour with her second child; she had been in hourly expectation of the birth for the previous twelve hours. I was called about eight o'clock in the morning. I found the os well dilated, and quite flaccid, as were also the vaginal walls and perineum. The pelvis was well formed, and there existed no mechanical impediment to the progress of the labour. The bowels had been well opened on the previous day. The tongue was slightly whitish. The pulse was soft, and about 56. She had had no food for twenty-four hours. The summer was a very hot one, and she lived in a confined attic room. She was therefore in a very exhausted condition, and her muscular condition was far under par. I encouraged her to make a breakfast of bread, eggs, and tea. On calling about an hour after, her condition was unchanged, and, thinking that she only now required some oxytotic agent to rouse uterine action, I administered a drachm of tinctura ergotæ. This was several times repeated during the forenoon without any definite result. I was unwilling to leave, as the condition of the parts led me to expect a very speedy termination to the labour, should even slight uterine action come on. Beef-tea, wine, spirits, and strong infusion of tea, were administered with evident relish, but also to no purpose. Digital irritation, even pinching the os, and external friction, gave no better result. Fearing post-partum hæmorrhage with such a condition of things, I was unwilling to use the long forceps. Every ordinary means of rousing uterine action I knew of I tried, but each and all failed to fulfil my expectations, and as other engagements were pressing, I resolved to try galvanism. Accordingly I sent for a galvanic machine, the appearance of which produced not a little amusement to the onlookers, although the patient eyed it with great apprehension. The first application was continued for two minutes. The uterus slowly responded, and after some seconds a moderate pain, the first for eleven hours, was induced, and continued to increase until the current was interrupted. The head descended to the brim. I now waited about ten minutes, and again applied the current for three minutes, which had the effect of bringing the head into the brim. After an intermission of five minutes, at the urgent request of the patient, who to my astonishment was pleased with the sensation produced, I again applied it, and in two minutes the head was rapidly born. By manual traction the birth of a healthy male child was completed in exactly twenty-three minutes from the commencement

of the application. I now directed the patient to keep her hands firmly pressed against the uterine tumour, and in ten minutes more I found the placenta in the vagina. Convalescence went on uninterruptedly.

The success of this case, and the evident pleasure with which its application was enjoyed by the patient, made me resolve to add galvanism to my list of agents for expediting protracted labours, especially for those specifically known as cases of *inertia uteri*. And as I experienced some difficulty in getting the female attendant to work the machine steadily, I procured one of Maw & Son's self-acting electro-magnetic machines, with its complement of appliances or electrodes.

In the spring of the following year, 1869, other two cases occurred, where similar conditions presented themselves.

CASE 2.—Mrs M., aged 35, a tall, well-made woman, the wife of a farm labourer, had parturient pains for about thirty hours. I saw her first on the afternoon of the second day. This was her ninth child; all the previous births were natural, the two last only being somewhat protracted, but were accomplished without instrumental aid. I found her very exhausted, and the pains had left her some hours. The os was well dilated, and the parts were quite flaccid. The head could just be felt a good way above the brim. For several hours I tried the usual means of rousing uterine action, but failed to elicit any response. The patient was very lethargic and disposed to sleep. But as the case seemed so simple, I was unwilling to let it become further protracted, and sent for my battery. Meanwhile I freely administered Liebig's extract of meat, spirits, ergot, and friction, but failed to rouse action. The first application of the current lasted ninety seconds, powerful uterine action was induced almost immediately, and the head came through the brim. The current was now interrupted, in imitation of natural pains, and uterine action ceased, but commenced again spontaneously at the end of two minutes. This spontaneous action, however, was very feeble, and at the request of the patient the current was again applied. The action now became so very powerful that the head was born in a very few seconds, and the birth of a female child was completed without further aid, the placenta following in about the usual time. In this case the after-pains, probably induced by the large amount of ergot administered, were so severe that they had to be allayed with opiates. She made a good recovery.

CASE 3.—Mrs M'D., 32, about 5 feet 8 inches in height, slender but well-made, was in labour with her seventh child. The labour had commenced about midnight, and I saw her early next morning. Uterine action was weak, and occurring only at irregular and distant intervals. The os was about the size of a shilling, but dilatable. There was no great mechanical obstruction, though the coccyx was unyielding and more than usually turned in, but

the pelvis was roomy. I had attended her in three previous confinements, which were natural, and required only digital assistance. I advised a good breakfast, which was somewhat reluctantly taken. In the course of the forenoon I returned, and found that there had been no action and no alteration in the condition of parts since the morning visit. I administered a large dose of ergot, and left, with orders to give another in the course of half an hour. On calling at the end of the hour, I found no change. Beef-tea, spirits, strong infusion of tea and coffee, porter, ergot, walking, digital irritation, and external friction, were employed from time to time, and several times during the day she slept for half an hour. Evening arrived, but still no action could be elicited, nor did the os further dilate. I now sent for the battery. The first application lasted for one minute. Slight pains were induced, but the os was inclined to contract rather than dilate. I interrupted the current for five minutes, and kept up digital dilatation of the os; meanwhile, during the next several applications, I kept my fingers in the os, so as to maintain dilatation and allow the head the more easily and with less obstruction to descend. The patient at first was very apprehensive of danger, and, thinking that there was no hope of her recovery, took a very affecting farewell of her husband, whose distress of mind during the operation was very audible from the next room. The patient now requested the application of the current more frequently than I considered advisable, and actually sent a message to her distracted husband "that she was enjoying it very much, and felt it doing her good." The descent of the head, however, was slower than I had experienced in the two previous cases. The tip of the coccyx offered some slight obstruction, but I persevered in the application, and in 35 minutes the head was born, and the birth of a large male child completed by manual traction. The placenta had to be removed, and there was a slight tendency to hæmorrhage, which ceased, however, on the administration of spirits and the application of the binder. She made a slow recovery, which I accounted for by the exhausted state of her system; her family having followed each other at about 14 months' interval.¹ This woman always recovered fast after her confinements, thus showing that she had good recuperative powers.

¹ I may state that this patient was delivered 18 months after this of twin daughters by my successor, Dr Stephen, of Fettercairn. He states that "the only peculiarity of the case was weak action, both before and after the birth. I gave ergot, which stirred up the pains, and the children were born rapidly and without any difficulty, both coming by the head. The womb was flaccid, and I had to grasp it for a considerable time. Notwithstanding the loss of a pretty large quantity of blood, and her previous weak condition, she has made a very good recovery. I hear to-day (2d July 1873) that one of the twins is dead. It was very small." She was on this last occasion longer than her average time between the conceptions, consequently she may have gathered rather more strength; indeed, less would be required for the expulsion of foetuses, small even for twin daughters. The flaccid, worn-out condition of the uterus again required both an oxytocic and mechanical stimulant.

These cases, though few in number, are sufficient to convince me that for some cases of labour we have in galvanism a potent power for good. That the results I obtained were due to galvanism I have not the least doubt. That the force evolved by the uterus was galvanic I doubt, but that galvanism roused and kept up a power inherent in the uterus, I think cannot be denied by the most sceptical. Ramsbotham, of London, in 1834, used galvanism, and was favourably impressed with its use in obstetrics and gynecology. Five years later, Killian invented a galvanic forceps, and after six years' use gave it up, the results having fallen short of his expectations. In 1844, Hoeninger and Jacobi used it for the induction of premature labour. To Radford, of Manchester, however, belongs the merit of first making a practical acquaintance with its use in obstetrics, and he became favourably impressed with its usefulness. The late Sir J. Y. Simpson tried it in eight cases, but considered it useless as an oxytocic agent, and of no utility in parturition. The method of application used by Radford and Sir J. Y. Simpson seems to have been suggested by Killian's galvanic forceps, viz., that of direct application of the electrode to the os and vaginal walls. Sir James held the opinion that if the galvanic machine had any oxytocic influence at all, it was not by the galvanism *per se*, but by the irritating influence of the metallic instrument used—the catalytic action, if I might so use the term. He held the opinion advanced by Jordan that the uterus, like the dartos in the male, being of similar structure, is incapable of being excited to contraction by the agency of galvanism. That the presence of the instrument in the vagina had some influence on the action of the uterus I do not venture here to deny, but that galvanism has an influence on the action of the uterus in intensifying its expulsive power, either directly or indirectly, I think the cases recorded above go far to prove; and the more recent experiments of Oser and Schlesinger stamp the assertion with authority, for they have demonstrated that electrical excitation of the central end of a spinal nerve calls forth, in from five to fifteen seconds, energetic contractions of the uterus.

The method of application I have adopted still further proves that contractions of the uterus occur by the use of galvanism without introducing any instrument into the vagina. I might ask, If the contractions are caused by the presence in the vagina of the metallic electrode only, why make use of galvanism at all? Why not treat every case of *inertia uteri* by inserting into the vagina a piece of metal? If the catalytic theory be correct, then this treatment should be legitimate and successful. I am not aware that the theory has had any practical outcome.

My method of application is as follows:—One of the electrodes, a flat piece of metal the size of two hands opened, and joined in the line of their long axis, and curved to fit the transverse abdominal curve, is applied to the abdominal parietes and retained by the patient's hands, a layer of blanket being between the metal and the

fingers to prevent the current entering by the fingers. The other pole is placed against the perineum, and retained by folds of blanket or a small pillow. The force can be moderated or increased at will, by the left hand of the accoucheur working the "regulator," while the right hand manipulates the child's head. The electrodes are now attached to the machine, and the circle being thus completed, the current is set on, and in obedience to a law in electro-physics, takes the shortest and most direct route from the abdominal to the perineal electrode, or *vice versa*. The uterus lying in this direct line shares in the influence; and in obedience to another law, its fibres contract, and in so doing aid in expelling the uterine contents, its action being limited by the electric force evolved, the quantity and condition of the muscular fibre of the uterus, and the calibre and patency of the passages. In this way the contracting or expelling force acts in the direction of the line of axis of the uterus. The induced contraction of the abdominal muscles must also exercise a very considerable influence over the expulsive process, this contraction lessening the size of the abdominal cavity and forcing outwards its contents. When the head descends and comes towards the perineum I shift the perineal electrode on to the coccygeal region, thus exercising a *vis a tergo*, and relieving the perineal muscles of their tonic contractions, which now become flaccid and permit dilatation. The head is thus born in the proper line of the axis of the outlet, with freedom. Possibly the action might be as powerful if the electrodes were placed anywhere on the abdominal parietes; but I prefer to place the one in front and the other over the coccyx, so that the action may be directed as nearly as possible in the long axis of the uterus and the outlet. If one of the poles of the battery were applied to the abdominal parietes, and the other to the os uteri, the whole uterus would be thrown into tonic contractions, the os would consequently be narrowed, and labour obstructed. In the cases recorded above, however, the current was not of sufficient strength to penetrate the whole body of the uterus, except in Case 3, where the os contracted slightly.

The electrodes I use for obstetrical purposes are two in number—the abdominal and the perineal. The abdominal is formed of two separate blades, each 6 inches long, 2 inches broad, and about $\frac{1}{8}$ th inch in thickness, slightly curved, transversely and longitudinally, to fit the curve of the enlarged abdomen, and joined together by a hinge, which permits of their being separated for facility of carrying. The perineal is a solid or hollow ovoid, 4 inches in longitudinal diameter, $\frac{1}{8}$ th inches transverse, and $\frac{1}{4}$ inch in thickness. Instead of being a hollow ovoid, it may, for lightness' sake, be of a scaphoidal form.

If the waters have escaped too soon, the parts should be well lubricated, to facilitate the descent of the head. In cases of *inertia uteri* the child will usually be born in a very few minutes after the application has commenced. But should a longer time be required,

then the application must be intermitted, so as far as possible to imitate the natural pains. Were it otherwise, the same consequences might occur as sometimes happens when ergot has been indiscreetly administered, viz., too violent and continuous action, with the risk of rupture or stoppage of the placental circulation and the death of the child.

Regarding the use of this agent, galvanism should be used when the os is dilated or dilatable; when the uterine action has ceased, and cannot be roused by stimulants, friction, or other oxytoxic agents; when there is evidence of uterine action becoming weak, irregular, or intermittent—time will thus be saved; when there is post-partum flaccidity of the uterus, or post-partum hæmorrhage, not caused by hour-glass contractions. In the latter case, I should first administer a 15 or 20 gr. dose of chloral (an excellent co-ordinator (?) of nerve force), wait for some minutes, then apply the current. In short, it is necessary and useful when there is simple, slight, additional expulsive force required, that is, in cases of *inertia uteri*, with a well-formed pelvis.

It might also be used for the expulsion of clots or air, conditions very troublesome, and often giving rise to grave symptoms in stout women during post-partum convalescence. It might be used with advantage in cases of uterine hæmorrhage caused by the pressure of fibrous or other tumours, or in their expulsion. Were an instrument made to encircle the tumour, and the current applied sufficiently long to stop the circulation, coagulation might take place in the vessels, and the tumour might die for want of nourishment. Ergot could not be given in doses sufficiently large to accomplish this without grave risk, but galvanism can be localized. It is also free from the risks attendant on the knife. Of course, these later remarks are entirely speculative, and my sanguine hopes of its practical application in these cases may be discouraged by the opinions of men of more extended experience; but my success with this agent in these cases of *inertia uteri* has emboldened me thus far to trace its probable application.

I would recommend galvanism, therefore, in cases of sluggish uterine action, for its use would save the time of the accoucheur, and shorten the labour of the mother. Its use in the latter case cannot be over-estimated, when we remember the frequent baneful effects of protracted labours. Its use at the time, and by the method I have indicated, is not attended by any danger to the mother or the child. It is pleasant to the mother, and hope is revived within her when she feels that its application is doing good. They all expressed themselves agreeably surprised.

Some may ask the question, "Why not use the long forceps in these cases?" My answer is, that even in the most skilful hands the forceps is not free from danger to both mother and child, immediate and remote. There are constantly present the risks of laceration and undue compression. These are accidents, though happily

rare, which sometimes occur, and although they are rare, there is a certain demoralizing influence or dread in the minds of some mothers at the idea of the "irons." I have known mothers on whom the dread of the forceps acted very deleteriously, even though they had been used on a previous occasion without any untoward event. At the same time, I am aware that many mothers hail with confidence the advent of the accoucheur armed with chloroform and forceps, knowing that by their use many hours of futile labour are saved. Although my experience is but limited, I cannot see any danger whatever, and none has been recorded, in the use of the galvanic current at the time and under the conditions I have mentioned.

Again, I may be asked, "Why not turn and deliver?" My answer is, that I had already used manipulative measures, to which the uterus refused to respond, and I had no guarantee that the uterus would contract after the child was extracted. I might thus risk the woman to flooding; whereas, by using galvanism, the emptying of the womb was made to depend on the womb itself, thus minimizing the risks of flooding.

Some, again, may ask, "Why not give opium, and induce sleep, and thus allow the strength to gather?" My answer is, that it may be too long in taking effect, and the head may thus be allowed to rest too long in one place, and give rise to sloughing of the parts; or, as I have frequently seen, action rapidly supervene, and the child born before my return. One cannot always calculate exactly the amount of exhaustion present, and is therefore unable to estimate the dose or the rapidity of its action. Galvanism is more certain.

I need not describe the *rationale* of the galvanic current in causing muscular contractions, either in voluntary or involuntary fibres. I am aware that we possess agents powerful enough to produce muscular contraction, but none so quick or so powerful as the galvanic current, and which can be localized with so much certainty, and none in the use of which the risks of injury are so few, unless the current be very powerful indeed.

One may doubt that the current can extend in an appreciable degree through the parietes of the abdomen directly to the uterus, and it is questionable if there is any special virtue in placing either pole more than the other upon the parietes. The galvanic current is not a co-ordinator of the distribution of nerve force, but a contractor of muscular fibres through the medium of the nerves, and, if applied locally, will induce the fibres within the radius of its influence, or within the same peripheral area, to contract. In no case does it induce muscular relaxation, hence the impropriety of using it *per se* to induce premature labour, or hasten it in the first stage. In the first stage the force to be overcome is the contractile power of the fibres of the os and cervix, or lower parts of the fundus, and if the galvanism is kept up long enough to induce these fibres to relax, it is accomplished by their becoming paralyzed by exhaustion—an un-

natural method ; and the same power has been at work on their antagonistic fellows of the upper parts of the fundus. Complete uterine paralysis is likely to occur—a condition not wished for—thus adding an unnecessary factor to the delay and danger of the parturient state. In all likelihood this was the cause of the delay and the death of the child in Case 8, recorded by Sir J. Y. Simpson, for, after its use had been abandoned for twenty-four hours, labour came on, and was completed without aid, though the brim was said to be narrowed. We must not forget, however, that the fibres of the fundus are much more numerous than those of the cervix, and if the electrode is not applied directly, they are much more likely to retain their power longer than those of the cervix ; and this is another reason why the electrode should not be applied directly to the os or its neighbourhood.

In Sir J. Y. Simpson's cases no reference is made to the size of the os during the periods of observation, consequently his experiments lose much of their value. I believe its action is analogous to that exhibited when the uterus is affected by irritating the nipples, and might be called "the transformation of reflex action." The influence is directed to the uterus, already the locality of irritation. Schlesinger has arrived at the conclusion that "the path by which the excitation issuing from the brain reached the uterus was certainly in no part through the nerve-plexus surrounding the aorta, but in part also through other though not yet satisfactorily demonstrated channels."

Sir J. Y. Simpson said that he did not find any unusual tonic contractions of the uterus during the application. In my cases, as no pains existed before the current was applied, and were present when it was in operation, and absent when intermitted, it is clear that tonic contractions did result, and were something more than a coincidence. Again, when the current was completed, there was distinct defining of the uterus, and a very considerable degree of tension in the protruding membranes during the application of the current.

In conclusion, I may add that in recent works on medical galvanism—a therapeutic agent only emerging from the territories of the empiric, and but reluctantly adopted by the profession—I find the subject of its obstetrical use altogether omitted.

Should I again meet with cases similar to those recorded above, I shall not wait the doubtful action of internal remedies, but at once apply the current.

ARTICLE IX.—*Excision of the Thyroid Gland.* By PATRICK HERON WATSON, M.D., F.R.S., F.R.C.S.E., Lecturer on Surgery and Clinical Surgery, and Senior Surgeon to the Royal Infirmary.

THE operation of excision of the thyroid gland has no claim of novelty to render it attractive. It has been practised previously by different surgeons, notably by the late Mr Liston, and the ill success which attended upon the attempts to extirpate this organ when most requiring operative interference, has served to deter most writers of the present day from recommending its adoption. Before practising this operation, I had twice assisted at operations where a partial extirpation of the hypertrophied organ was adopted. In one case under the care of the late Mr Syme, where in a cystic goitre, after tapping and injecting the cyst, aneurismal signs manifested themselves, he laid open the pulsating sac, and failing to arrest the hæmorrhage, which poured out in a rapid stream, he seized first one side of the sponge-like sac and then the other, cutting away as much of the walls as could be exposed. In this case the hæmorrhage proved unrestrainable in spite of stuffing the cavity with sponges, and stitching the margins of the incision together over them, and the patient died in our hands. In the second case, Professor Spence extirpated a tumour of the isthmus of the thyroid with complete success, but with great bleeding attending upon the division of the vascular connexion. The hæmorrhage which complicated both of these cases, was certainly such as to make any one timid in adopting such a proceeding.

In May 1871 I was consulted by the friends of a young lady, who for many years had been affected by a tumour of the neck. It was central, prominent, of the size of a China orange, moving with the trachea, corresponding to its upper part, and of an elastic, solid consistency. It was a source of great disfigurement, and was gradually increasing in size. I recognised its thyroidal nature, and recommended its removal by excision; being tempted to advise this from its limited central position, and its prominence, which was obviously due to the separation of the sternohyoid and thyroid muscles in the progress of its growth.

The operation was effected by a central linear incision, through which the surface of the tumour was readily exposed, with scarcely any bleeding. But no sooner were the lateral attachments on one side divided than blood gushed in a copious stream both from the tumour and from the vessels. The vessels were secured with some difficulty, and a ligature, embracing one half of the tumour, arrested all further flow. To escape from further trouble from bleeding, before dividing the vascular connexions upon the opposite side, I passed two preliminary ligatures beneath the superior and inferior thyroid vessels, and having tied these ligatures, completed the

operation without any further loss of blood. The patient made a rapid recovery. This easy method of controlling the circulation towards the tumour impressed me with the idea of adopting this plan of mediate deligation as a preliminary to the extirpation of the gland.

In the course of the same month (May 1871), a patient was admitted under my care in the Royal Infirmary, affected with a multi-ocular cystic goitre. She had previously been under treatment, when the single cyst, of which the tumour then apparently consisted, had been tapped and injected with iodine. For a time this had seemed to have arrested the development of the gland, but it again gradually enlarged, until at the time of her readmission the swelling was of the size of two fists. The patient, at the same time, was anæmic, and affected in a slight degree with exophthalmia.

In her case, I operated by a long linear incision, which sufficed to expose the upper and lower margins of the tumour. After dividing the skin and cellular tissue, and opening the fascia over the interval between the sternohyoid and thyroid muscles, carefully avoiding the fascial sheath of the thyroid gland, I carried my forefinger and thumb over the margin of the tumour at its upper and right-hand corner, and feeling that I had the vascular connexions of the tumour, with the right superior thyroid artery in my grasp, I introduced an aneurism needle through the fascial sheath in the middle line, bringing it out again at the right side of the level of the equator of the tumour. A ligature was passed through the eye of the needle, and when the needle was withdrawn, the ligature was left in its track. This ligature was confided to an assistant, and held aside. The needle was again passed in the situation of its former emergence, guided by the finger, and then passed beneath the right inferior margin of the gland, so as to include all the right inferior thyroidal connexions. The ligature was similarly withdrawn along the track of the needle. The same process was repeated upon the left side, a ligature being carried beneath the left superior and inferior thyroidal connexions, together with their delicate investing fascia. These four ligatures were then separately tied, so as to secure the vessels included within their cellular sheath as near their origins as was possible; the further separation of the tumour was effected by curved scissors. As the right superior thyroidal attachments were divided, the ligature came away in the hands of the assistant to whose care it had been confided. A gush of blood took place, but was at once stanchèd by means of a sponge thrust into the wound. On cleaning the parts, and drawing the edges of the incision widely asunder, the vessels were again secured by a single ligature.

The wound was closed by sutures, except at the lower angle, where a portion of large-sized drainage-tube was inserted to secure the thorough evacuation of all blood, serum, or pus during the after-

treatment. The surface was dressed with a pad of oakum, retained in position by plaisters and a cravat composed of a muslin handkerchief folded upon a piece of buckram, after the manner of an old-fashioned stock. The loose and flaccid skin had in two days become adapted to the parts beneath, and showed no signs of any redundancy. The wound was healed in a fortnight, except where the ligatures hung out. One of these, after the lapse of a month, required for its removal to be forcibly drawn out until the knot appeared, and its loop divided on one side.

The next case, one of multiple cystic goitre, was operated upon in the Chalmers Hospital, in the autumn of 1871, and presented no peculiarities, except that the ligatures employed consisted of strong catgut, which were cut short after the division of the attachments of the tumour, and its removal.

Here the healing process was completed in three weeks, and the patient, who had been sent to my charge by Dr Howden, of Montrose, returned to her duties as an asylum attendant. In this case, the anæmia and exophthalmia which were present on admission, markedly diminished after the operation.

The next case was that of an Irishwoman affected with a multiple cystic goitre. She was admitted to the Chalmers Hospital in the beginning of 1872. The operation was performed exactly as in the last case, and was followed by equally favourable results.

The last case, a patient from Leith, was admitted to the Royal Infirmary in February 1872. This was also a case of multiple cystic goitre, which had grown steadily for twenty-three years. Its commencement dated back to the commencement of menstruation. It was of the size of a large fist. It pulsated distinctly, and was accompanied by anæmia and exophthalmos. The operation was performed upon the 1st March 1872 in precisely the same manner as already detailed. The wound, however, was not entirely closed for six weeks after the operation. The retention of a small portion of sloughing texture within the wound was the occasion of this delay. An anomalous condition of matters manifested itself in this case after the healing was complete. A pulsating swelling about the size of a small hazel-nut developed itself in the situation where the right superior thyroidal vessels had been included within their cellular sheath in the grasp of the ligature. This gradually increased in size till it was as large as a small walnut. It then presented all the characters of an aneurism with a venous communication, manifesting the rattling of a shot-like thrill, and the *bruit de diable* in a marked manner. After continuing for about three weeks, it entirely disappeared, without any treatment being employed except rest in bed. The result attained in this case I had an opportunity of exhibiting to the Society one evening at the commencement of the present session. So far as I am aware, this method of operating is original. Mediate ligature of the thyroids has, I am aware, been proposed and employed as a means of curing

—or, at least, arresting—the growth of the goitre, though its practical employment was not found to be attended by such results as to lead to imitation. The operation was further effected by four separate incisions, each made in relation to the vessel to be secured. In the operation I have now the honour of submitting to the Society, the deligation of the vessels is only a preliminary to the excision of the tumour, and is effected through the same incision as that by which the extirpation of the goitre is made.

To attain the best results—(1.) The external incision should be very free, extending from the larynx to the notch of the sternum if the tumour is large and spreads widely in a lateral direction. (2.) The vessels, arterial and venous, in the superficial incision should be secured as they are divided, to avoid any obscuration of the parts through oozing going on. (3.) The fascia should be as freely opened as the skin. (4.) The investing delicate fascial sheath of the thyroid should be left undivided until the mediate ligature has been effected of the vessels included in their fine cellular sheath. The sheathing, fascial, or cellular capsule of the thyroid gland is only a prolongation of the sheath of these thyroidal vessels. If the capsule is opened, then, in pushing aside the soft parts to disclose the outline of the tumour, this delicate sheath is apt to glide off the surface of the thyroid gland; and should this occur, the gland may readily be detached from the vessels by comparatively gentle handling, and thus copious hæmorrhage, difficult of restraint, may be occasioned. (5.) After the mediate ligature of the thyroidal vessels in their sheathing cellular envelope, the cellular capsule of the thyroid gland should now be opened, and the attachments which still retain the goitre in its position carefully divided by means of scissors, curved and blunt-pointed. There should be no tearing away of the gland—no pushing parts aside with any roughness of manipulation. (6.) Should bleeding occur, it must be recollected that it must take place within the cellular sheath of the vessel and its prolongation upon the gland in the fashion of an investing capsule, and that, if the vessels are to be tied, they should be secured along with the cellular sheath. Without this sheath these enlarged trunks will be found so fragile as to risk being cut by the ligature; while any attempt to reach the bleeding mouths will usually be balked by the infiltration by clot of this cellular envelope.

I need hardly say, that a method of operation such as this, which is easy and rapid of execution, and, as we have seen, devoid of risk, so far as I have yet tested its employment, holds out great attraction to the surgeon over the use of the seton or the caustic *feches*, in the treatment of these affections of the thyroid gland.

Part Second.

REVIEWS.

Cancerous and other Intra-Thoracic Growths, their Natural History and Diagnosis: Being the Substance of the Lumleian Lectures delivered before the Royal College of Physicians of London. By JAMES RISDON BENNETT, M.D.; Fellow, Senior Censor, and Representative of the College in the General Medical Council; Consulting Physician to St Thomas's Hospital, etc., etc. With five Plates. London: Churchill: 1872. Pp. 189.

THIS is a very valuable contribution, by a well-known and highly respected physician, to the clinical history of the diseases of which it treats. The diagnosis of pulmonary and mediastinal cancer has only been carefully studied and satisfactorily illustrated during a comparatively recent time, while opportunity for enriching our stores of knowledge on several important points, in relation to these diseases, still exists. Dr Bennett thinks that, for clinical purposes, the various forms of intra-thoracic cancer may be conveniently arranged in three classes. 1. Those in which cancerous deposits, usually in masses varying in size, are disseminated through the lungs, having more or less analogy with disseminated tubercle, but without inducing any change in the intervening lung. 2. Those in which the cancerous growth is more localized; attains, for the most part, to a greater size; and tends to important ulterior changes, such as ulceration and gangrene. 3. Mediastinal or other tumours, inducing pressure on the tubes, vessels, and nerves, with all its important and very various consequences. To a consideration of each of these, a section of the present work is devoted, and a fourth section is occupied with the discussion of non-cancerous intra-thoracic growths.

In a brief introductory chapter, Dr Bennett offers some interesting observations of a general character. He says, for example, "Cancerous disease occurring primarily within the chest, is not so rare as was at one time supposed, although there are probably many physicians who have never seen a case." That it was supposed to be rare cannot be doubted, when we find another distinguished writer, of the same name as our author, thus expressing himself:—"This is the only case of cancer of the lung which I have ever met with; so that, I presume, the disease rarely attacks this organ in Scotland."¹ Equally true, however, is the statement that the disease is not of the rarity which it was formerly believed to be. We venture to

¹ On Cancerous and Cancroid Growths, by John Hughes Bennett, M.D. Edinburgh, 1849. See page 45.

affirm that there are few hospital physicians, whatever their sphere of duty may be, who have not frequently encountered it, although their experience may have fallen far short of Dr Bennett's. A reference, however, to the literature of the subject, and particularly to the interesting essay of Dr Cockle, is decisive on this point.

The following is another important *introductory* observation :—
“There is reason to believe that cancerous disease is steadily on the increase in this country.” Reference is made by Dr Bennett to the observations of Mr Moore, founded on the Registrar-General's returns, that the increase in deaths from cancer in London, coincident with the increase of wealth and the wellbeing of the population, amounts to two hundred *per annum*.

Hitherto it has been generally supposed that the right lung is far more frequently affected with cancer than the left; but of thirty-nine cases tabulated by Dr Bennett, the left was the principal seat in fourteen, the right in nine only, whilst of the remainder, either both lungs were affected, or the disease was confined almost entirely to the mediastinum. Neither is the supposed preponderance of males over females affected by pulmonary cancer borne out by Dr Bennett's cases, nineteen having been females, and twenty males. In reference to this point, we find Dr Walshe writing:—“The general law, which assigns a vastly greater share of cancerous disease to the female than to the male sex, is infringed in the special instance of the lung. Of one hundred and two cases (forty-four collected by myself, forty by Aviolat, and eighteen by Wunderlich), the proportion supplied by males was fifty-eight; by females, forty-four.”¹ The tendency on the part of pulmonary cancer to affect members of the same family has not fallen under the observation of our author, but is noted by Dr Walshe. “Two brothers,” the latter states, “of closely similar form and general appearance, began to suffer from symptoms and signs, readily traced to pulmonary cancer, when they had almost reached their fortieth year. In both, the disease began at the root of the right lung; and in both, the peculiar red jelly-like expectoration occurred.”² In our own experience, an example of the same nature, but even more striking, has occurred. A youth of about eighteen years manifested the unequivocal symptoms and physical signs of pulmonary cancer, and the disease in time ran its invariably fatal course. Eighteen months after his death, an elder brother, living under the same roof, became similarly affected. He died in his twenty-eighth year; and within a year was followed to the grave by the father of both brothers, aged about sixty, a sufferer from the same disease. In respect to age, the oldest of Dr Bennett's patients was seventy-two; the youngest, eleven. Four were between thirty and forty; three, between forty and fifty; and seven, between fifty and sixty. We find the following very sensible remarks on diagnosis in Dr

¹ A Practical Treatise on the Diseases of the Lungs, 4th edition, page 518.

² *Loco cit.*, page 519.

Bennett's introductory chapters:—"But the aid to diagnosis which the previous history of the case affords us in these secondary forms of the disease (following surgical operations for the removal of cancerous growths in other parts of the body—chiefly the breast) is no less important in the primary. Indeed, in every case of obscure thoracic disease, a careful investigation and study of the anamnesis is of the utmost importance. This, by way of exclusion, if not directly, will often lead us to a correct diagnosis, when it would be impossible from a consideration of the existing physical signs and symptoms alone. The diagnosis must, therefore, be studied in connexion with the natural history."—P. 7.

Encephaloid cancer is the form of disease by far the most frequently met with in the lung. So much so is this the case, as to justify Dr Beigel, in his interesting account of pulmonary cancer furnished to Dr Reynolds's System of Medicine, in almost limiting his observations to pulmonary encephaloid. Scirrhus and colloid cancer of the lung are very rare.

Under the head of Cancerous Deposits disseminated through the Lungs, Dr Bennett gives some interesting cases, with carefully recorded post-mortem appearances. The first of these, in a young girl of fifteen years of age, is intended to illustrate the remark, that, although miliary cancer is rare as a strictly primary affection of the lungs or pleura, it does sometimes occur as the only form of cancerous disease within the chest. The remaining instances of a similar nature recorded in section 1 are accompanied by some excellent suggestions in regard to diagnosis. Section 2 is occupied with the consideration of cancerous disease limited to certain parts of the lungs. Under this head, an instance of death resulting from hæmorrhage—a rare termination of pulmonary cancer—is given. The subject of mediastinal cancerous tumours is handled in section 3; and from a perusal of that chapter more particularly, as well as of the concluding section devoted to a consideration of the non-cancerous intra-thoracic growths, we have been led to form a very favourable opinion of Dr Bennett's work.

A System of Midwifery, including the Diseases of Pregnancy and the Puerperal State. By WILLIAM LEISHMAN, M.D., Professor of Midwifery, Glasgow. James Maclehose: 1873.

A Manual of Midwifery, including the Pathology of Pregnancy and the Puerperal State. By Dr KARL SCHROEDER, Professor of Midwifery in Erlangen. Translated into English from the third German edition, by CHARLES H. CARTER, M.D., etc. London: J. and A. Churchill: 1873.

THE profession is to be congratulated on the simultaneous appearance of two great and excellent works on midwifery. Our readers

will have gathered from several of our notices on the obstetric literature of recent years, that while we admire the zeal and diligence of British authors, we recognise in them generally a lamentable meagreness, much ill-considered spasmodic working, much foolish printing, evidently for mere publication's sake. Illustrations of all this we have in manuals of midwifery which have received from eminent metropolitan journals such praise as makes us blush for the silly reviewers, evidently young lads either grossly incapable, or paid directly or indirectly for their contemptible attempts to mislead the brethren. The two works now under our notice will raise the character of British authorship at home and abroad. No doubt they have faults. Who is equal to the production of a vast systematic work, carefully elaborated in all its parts, up to the day? For ourselves, we cannot too much admire the patience, and zeal, and intelligence that are expended by gentlemen who, after hard days' work in actual practice, fill up their *horæ subsecivæ* by still harder work for behoof of students and practitioners. Such men will have their reward. Leishman and Schroeder deserve it well for subjective as well as for objective reasons (to speak like a German).

A leading London journal has recently been unconsciously proving its own charming innocent Cockneyism by pointing out for the benefit only of its weaker readers the mere provinciality of the Edinburgh school—indeed, of all schools but that of London. This sort of nonsense, inconsistent as it is with history or with the present state of the case, is believed by no one except a few writers and readers of such London journals; but the disease of which it is evidence affects the minds of many classes of literary gentry. We read not long ago the speeches of a gang of famous writers who after dinner were bidding adieu to one of their number who was about to be exiled to a provincial city, and we found them most heterodox, declaring that London fogs and smoke were the best modern source of literary inspiration, not mountain and flood, nor dell and prattling brook. The two works now before us will not lend support to this way of thinking. Leishman, as a mere writer of English, and as an obstetrical author too, at least takes place with the best systematic authors of the century. His predecessor, Burns, produced a standard work, which is far superior to anything in the language published at the time or for long afterwards: it went through ten or more editions, and is still extensively used. We shall not speak so decidedly of our contemporary Leishman, but we may boldly say that Burns has a worthy follower. Bred in Glasgow, Leishman has now produced two books which give him a leading place in British obstetric authorship. When Schroeder's book first appeared, we directed our readers to it, and we did not mislead them. It has already run through three editions, and been translated—and it well deserves to be—into English, and we believe also into Russian. Schroeder does not belong to Berlin or Vienna, or any great school, but to the minor University of Erlangen.

We regret to say that we cannot speak favourably of the rendering of Schroeder. We did not desiderate a fine flowing translation, and we cannot be content with one which is often erroneous and misleading. Of errors we might give many examples, but in order to illustration we shall describe only one. Presentation and position are carefully defined; and in an adjoining page, the one term is used erroneously instead of the other; and this confusion is not in the original. But this is not the only kind of fault of the translation. We find the greatest liberty taken with the text. At one place references are inserted, at another they are omitted. At one place the valuable notes of the author are entirely omitted, at another they are given in part, at another, again, given in full: and such notes or parts of notes are inserted in the midst of the text, according to the caprice of the translator. All this ought not so to be. We hope this edition will soon be sold out, and that we shall have an improved one—full and faithful, if not elegant. The book is so admirable a compendium, and so fitted to be useful to British readers, that we trust our wishes will be realized. Schroeder has a good many additions and few corrections to make for his next edition, which will form the opportunity for our excellent, enterprising, Messrs Churchill. Carter's work, good as it is in some respects, will be useful as the basis for future corrections and additions.

The work of Leishman gives an excellent view of modern midwifery, and evinces its author's extensive acquaintance with British and foreign literature; and not only acquaintance with it, but wholesome digestion and sound judgment of it. He has, withal, a manly, free style, and can state a difficult and complicated matter with remarkable clearness and brevity. We would not imply that Leishman has nothing to correct; and here again we give one little sample, not of an error, but of a confusing and unauthorized use of a very convenient word. The "conjugate" is appropriately and universally used to designate one diameter of the somewhat elliptical brim, and its value and appropriateness are lost if the application is extended. This Leishman does, we think unfortunately, using the word to designate all the diameters which many Germans conveniently call *sagittal*,—a shorter and more euphonical term than antero-posterior. The description of the changes undergone by the cervix in pregnancy, parturition, and the puerperal state, is not satisfactory. The treatment of the great subject of hæmorrhage is a fine example of what our author can do. Of course the mechanism is well done, distinctly given so far as it goes, but we desiderate many modern developments. The great subject of puerperal fever is at present unsusceptible of any definite treatment,—it is in a chaotic state; yet our author does not shine in this part.

Were we to go on with these two works, we would have to write a book, so diversified are their contents, and so attractive. We conclude by congratulating Glasgow and Erlangen on giving good text-books to the metropolitan schools of the world.

On the Scientific Value of the Legal Tests of Insanity. By J. RUSSELL REYNOLDS, M.D., F.R.S., Professor of the Principles and Practice of Medicine in University College, etc., etc. London: 1872.

Responsibility and Disease: An Essay. By J. H. BALFOUR BROWNE, Esq., of the Middle Temple and Midland Circuit, Barrister-at-Law. London: 1873.

It occasionally happens that a reviewer who collects a few books or pamphlets on a disputed subject, finds that his labour is much lightened by some of the authors reviewed doing his work upon one or other, and here is a good example. Dr Russell Reynolds reads a paper on the "Scientific Value of the Legal Tests of Insanity" before a Medical Society, which he afterwards publishes in the form of a pamphlet. Mr J. H. Balfour Browne republishes an essay upon "Responsibility and Disease," which had appeared in the *Law Magazine and Review*, adding some remarks upon the paper of Dr Reynolds. Any one anxious to know what can be said on the subject, both from a common medical and legal point of view, ought to get these two pamphlets and study the arguments on either side. The one essay cannot be fairly studied without the other; and in this way Mr Browne may be said to have done a service to Dr Reynolds, for we do not think that viewed alone the pamphlet of the latter gentleman merits much attention. We do not deny that it is graphically written and full of finely-drawn distinctions and thoughtful remarks; but we can scarcely imagine that Dr Russell Reynolds himself, if he had to write the paper over again, would have put the thing in the same form. Even if some of Mr Browne's thrusts might be parried, yet he has hit the learned Professor in so many vital places that it is no use carrying on the conflict.

It may be true in one sense that "in a legal inquiry the end to be obtained by the lawyer is identical with that which the physician sets before himself, viz., the diagnosis, discrimination, or distinction, in a particular case, of insanity from sanity, and from feigned disease;" but then they make this inquiry for very different objects. The doctor's inquiry ranges over the etiology of the disease, its whole course, its probable termination, and the effects it may have on the bodily as well as the mental health of the patient; the lawyer merely wishes to make out whether a certain person can be proved to be civilly capable and criminally responsible. To him a man is either mad or not mad; either able to manage his affairs and make his will, liable to be punished as a responsible being, or to be deprived of his liberty as a lunatic. "We cannot draw the line," writes Dr Reynolds, "between health and disease in such chronic maladies, and yet, according to the legal theory, it must be drawn, and men must be ranged, all of them, on one or the other side."

Dr Reynolds shows how difficult—how impossible it is to draw any line without doing injustice to somebody; a consideration which he uses as a valid argument against the state of the law. This we imagine is one of the most common fallacies of our time. Everywhere we hear proposals to do away with laws which, while they regulate great difficulties, and protect society from great evils, yet do injustice to exceptional cases. The pursuit of absolute, poetical justice, however pleasing in theory, is a refinement which our courts never can reach. Nor is this, we imagine, a difficulty confined to the lawyers. Medical men, too, are often called to act upon rough rules, leading them to probabilities, which they know in speculation to be only probabilities, but upon which they are compelled to act, if they are to remain in the practice of their profession. The physician is often compelled to act upon a diagnosis which he knows to be uncertain, and which, when his patient has passed beyond the reach of his art, he may find to have been a mistake. We have heard the most experienced surgeons confess at the operating-table that they had operated under a false diagnosis.

Mr Browne makes use of a familiar consideration, which, if properly held in view, is quite enough to enable us to refute a good deal of the nonsense of the day. "The growth of intelligence is as gradual as the growth of the body; and as the management of property and affairs presupposes the possession of intelligence, the law, to protect men from themselves, found it expedient to say, that unless a man had intelligence he should not have complete power over his property, and it consequently became necessary to draw a line between childhood and manhood, and the law did so, saying a man shall come of age when he is twenty-one years old. The scientific value of this test of intelligence is evidently very small. It cannot be said that a man is really able to enter into an intelligent bargain to-day which he would have been unable to enter into yesterday. Besides, some men have more intelligence at seventeen than others have at forty-five. The absolute necessity for some such rule, however, and the impossibility of devising one more in harmony with the facts of human development, are sufficient reasons for the existence of this rough test of mental power." Apply this argument, for example, to the following remark of Dr Russell Reynolds:—"It is not my purpose to enter upon the ethical question of the difference between capacity for civil and responsibility for criminal acts, although I think it would not be wrong in me to state my own conviction that any form of disease which should be allowed to deprive a man of liberty and disposing power in regard to property, should be admitted to excuse him from the responsibility for a criminal act. It seems to me illogical that a man should, for example, by reason of disease, be prevented from excluding from the benefits of his will a relative who has been unkind to him, and whom he may sincerely hate; and that he should yet be liable for penalties if he knocked down that obnoxious relative when he

planned to come into his presence for the purpose of cajoling or intimidating him into doing this or that."

If Dr Reynolds had remembered that a child is not held criminally responsible for actions whose bearing he cannot properly understand, but that a young man of twenty can be punished for a crime in a court of law, though he cannot dispose of his property nor execute a will, he would scarcely have treated it as unreasonable that capacity for civil and responsibility for criminal acts should be separated. In the passage quoted the case is not happily put, for it is not the object of the law to prevent a man disinheriting one who has used him ill, but to prevent a man of unsound mind making a disposition of his property which might exclude those who to a sane mind would appear most deserving. We can perfectly conceive of a man whose mind was so much disordered as to be unable to make a proper will, but who had sense enough to know that he would be punished if he knocked down any one without a sufficient justification, and who would be deterred from so doing by the fear of punishment. Dr Russell Reynolds shows, in a very clear manner, the fallacy of making delusion a test of insanity. There are many cases of insanity without delusion, but then Mr Browne shows that the law has already recognised the justice of what Dr Reynolds has argued.

Dr Reynolds does not propose any better plan of getting over the difficulties of this extremely difficult subject of the legal bearings of insanity, but wishes a conference of medical men and lawyers to meet to consider the matter; and we believe a committee so composed has been actually appointed. It is likely, then, that new tests will be made, or the old ones modified; and we hope that they will be an improvement on the old ones. Nevertheless we are sure that they will be exposed to innumerable speculative objections; and we are disposed to believe that it is better that our civil capacity and criminal responsibility should be in the hands of judges and juries than of medical experts, who are continually disagreeing from one another on important points of pathology as well as morals and law.

Medical men, we imagine, require to be summoned as witnesses in such matters; but, taking it all in all, it is better that they are not made judges. We are far from holding that anomalies in the law of lunacy do not exist, and are much tempted to treat the reader to some we consider especially flagrant; but it is likely he has had enough of the subject at present.

On Ovariectomy. By J. MARION SIMS, M.D., etc. D. Appleton and Co., New York: 1873.

THE author of "Silver Sutures in Surgery" jumped suddenly into deserved fame when he showed the curability of vesico-vaginal fistula. This pamphlet was a wild, extravagant, and not very correct production; very bad in taste; but all its faults were soon

condoned when the professional world found that the fistulæ were now brought into the rank of diseases ordinarily curable, and this for the first time. Our great surgeon laughed at Sims, good-naturedly saying he had known many modes of cure, but had never seen one effected. Before he died he recognised Sims's achievement with warmth, and cures were numerous in his immediate neighbourhood.

Subsequently, after coming to Europe, Sims published his "Clinical Notes on Uterine Surgery."¹ This work extended greatly his fame, demonstrated his ingenuity, illustrated his boldness and ability in operating, but, as a final result, it has not increased his good substantial reputation. It is not a scientific work, but a work of rashness and of much injudicious boldness. Besides, our author has become well known in European medical circles by papers, speeches, and otherwise, and there has not been much reason for satisfaction with the later career of one whose name will always be honoured among us as the great improver (along with his excellent colleague, Emmet) of the operation for vesicovaginal fistula.

Ovariectomy occupies now, and justly so, the greatest place in the eyes of the surgical world; and apparently in consequence of this, Dr Sims has taken to dabbling in it, literarily to a great extent, practically to a moderate amount. Unfortunately for him (and we believe not altogether fortunately for the operators and for patients), ovariectomy has always been treated statistically, and Sims gives us no glimpse of his statistics. We assure him that, in ovariectomy, he need not expect great consideration unless he shows his hand.

He devotes much attention to two points, both well worthy of study. Both, indeed, have been carefully studied, and Sims contributes a little to their advancement. The two points are, that septicæmia is the great cause of death after ovariectomy, and that drainage of the peritoneal cavity through the vagina (long ago suggested by Dr George Keith of Edinburgh) is the best, if not sure, means to prevent it, letting off, as it does, the reddish serum from the peritoneal cavity, which Sims believes is the sign and cause of the septicæmia. Sims is very far from proving either of his points, and has not resorted to the only means of verification of his views. It is not sufficient to whip up a lot of cases with "reddish serum," and say they are cases of septicæmia. That can only be done by careful history, dissection, and other investigation of each case. Sims's way of getting his cases reminds us of a boy dabbling for apples in a bucket of water. Similar remarks may be made as to the efficiency of drainage. In the following paragraph Sims states his theory:—

"I do not pretend to deny that death may occur from shock, or from hæmorrhage, or from clot, or from exhaustion, or even from peritonitis; but I feel sure that those, independently of blood-

¹ Every gynecologist should study Scanzoni's admirable critique on a part of this book, published in his own *Beiträge* for 1870.

poisoning and its legitimate causes, are of comparatively rare occurrence. If, then, we have such an almost universal evil to deal with as septicæmia, and if that septicæmia is, in 37 cases out of 39, clearly traceable to the poisonous fluids effused in the peritoneal cavity, is it not self-evident that the indication, both of prevention and cure, is to draw off these poisonous fluids in the speediest and most direct way possible? My facts and premises granted, the conclusion is inevitable, and we must all say yes. But many will reply that this is nothing new, that it has often been done before, and many lives have already been saved by it."—P. 28.

Clinical and Pathological Observations in India. By J. FAYRER, C.S.I., M.D., F.R.S.E., etc. Pp. 648, 8vo. London: J. and A. Churchill: 1873.

DR FAYRER'S name is already well known to all who follow the progress of surgery in India, as the author not only of a valuable work on Clinical Surgery in India, but also of the magnificent folio describing and illustrating the poisonous snakes of India. In the volume we have now to review, he has collected a large number of miscellaneous papers on subjects of medical and surgical interest, chiefly the latter; the points which he thinks of most interest "having reference to those open questions, septicæmia, blood dyscrasia, and disturbance of the nervous system, as illustrated in the remarks on pyæmia, osteomyelitis, embolism, the formation of cardiac coagula, and urethral fever."

The book begins with the address delivered by the author as President of the Bengal Branch of the British Medical Association. The health of hospitals, ward space, antiseptics, vibriones, a very long list, chiefly of trifling cases treated by antiseptics (then in the oil and putty stage of development), two cases of death from urethral fever, and a note of the author's operation for reducible hernia, make up an address of extreme length, utterly devoid of point or precision, and one which we fear must have tried to the utmost the patience of the auditors, unless it was listened to in relays, or held as read.

Next we have an answer to Dr Braidwood's criticisms of Dr Fayrer's views on pyæmia. Certainly Dr Fayrer, like Dr Braidwood, seems to have been unfortunate enough in having plenty of opportunities of studying this malady.

The chief point regarding which these excellent observers are at issue is as to whether osteomyelitis when it occurs should be considered (as Fayrer does consider it) "as a distinct original and dangerous form of disease, liable to occur under peculiar circumstances," and *productive* of the most deadly form of surgical fever; or whether we are (with Braidwood) to regard osteomyelitis as merely a *secondary manifestation* of pyæmia, just as metastatic abscesses of liver

and lungs are. On the whole, we think Dr Fayrer has the best of it, some of his cases showing that, by early amputation at the joint above, it is possible to prevent the constitutional results of an osteomyelitis already begun. The cases which illustrate and confirm these statements are very numerous and interesting, though told with considerable prolixity of detail.

We next have a series of cases of death after operations in which fibrinous coagula were found in the right side of the heart. Dr Fayrer seems to consider these as the cause rather than the consequence of the act of dying, and would associate their formation with a previously altered condition of the blood, which he connects with malarious poisoning. The paper is a very suggestive one, will stimulate investigation, and repay perusal.

Connected with the preceding are the cases of gangrene from arterial embolism, of which a considerable number are recorded, nearly all in cachectic natives who had been exposed to malaria. One, in a European, recovered without actual death of the limb.

Some trifling little notices of rock-oil as a dressing of wounds, opium as an internal remedy in ulcers, and horsehair for sutures, come in rather oddly after the solemn pathological subjects we have been discussing.

Three cases of aneurism follow: one of true aneurism in a negro, for which the external iliac was tied with success; one of traumatic aneurism of the anterior tibial, treated successfully by plugging the wound with lint; and another of the popliteal, in which the old operation was performed, and the patient died after secondary hæmorrhage, with a fibrinous clot in his heart.

Four cases of tetanus are recorded; in one, a mild chronic one, section of the median nerve seemed to do good. In two more, tolerably mild and chronic ones, amputation of a toe and finger respectively were recovered from; while in a fourth, very smart one, excision of a cicatrix seemed not to produce the slightest benefit.

Some cases of gunshot wound are well recorded.

A careful synopsis of the cases of fracture treated by the author between 1859 and 1871 is given. The chief point that strikes the reader is the fearfully high mortality in compound fractures. In five cases of primary amputation for fracture of humerus, one recovered; of four secondary ones, all died. Of fourteen cases of compound fracture of forearm, nine died, four of them after amputation had been performed; and out of fifty-four cases of compound fracture of the leg, twenty-eight died. These results are notwithstanding the use of carbolic acid dressing during the later years.

A series of shark-bites are described. Eleven cases of tracheotomy give three recoveries, all (as is usual) in young adults, where the operation had been performed for some syphilitic ulceration. One case, in an elderly man, died, evidently from intralobular pneumonia and plugging of bronchi, the result of a small hæmorrhage into the trachea *during* the operation.

Paracentesis of the knee-joint is now so common as hardly to

deserve such a detailed account of two cases in which it has been done by Dr Fayrer, who does not venture on the subsequent injection of iodine.

The necessity for early operation in strangulated hernia in Calcutta is insisted upon, and twenty-seven cases detailed, of which sixteen recovered and eleven died.

Dr Fayrer then gives cases of Holt's operation, with a favourable opinion as to its merits.

Lithotomy is illustrated by fifty cases, which include several of the median operation, and three of removal of calculus from the urethra, which rather complicates the statistics. The results are rather disheartening, twenty-eight per cent. having died. This is to be ascribed to bad hygienic condition of the Calcutta hospitals, and the weak broken-down constitution of the patients.

A large and valuable series of scrotal tumours are next recorded—115, with 17 deaths. In one, the patient weighed 218 lbs. before the operation, and only 108 lbs. after it. He died of shock.

The results of operative interference in injuries of the head are very unfortunate; of seven cases trephined, all died.

Amputations also fare no better. Of eight amputations at hip-joint, only one recovered, and he was one of secondary amputation, after an amputation of the thigh. And the results of amputation of the thigh are still worse—three recoveries out of thirty-seven cases. Even in the leg, including amputation at ankle of *sixty-one* cases, only *twenty-four* recovered; and in the amputation of toes, arm, and forearm, the results are nearly as bad in proportion.

Dr Fayrer deserves much credit for the frankness with which he puts on record these most depressing results, and the hopeful spirit with which he informs us that these results, bad as they are, are not so bad as they used to be.

A good many other interesting cases and observations might still be quoted, but enough has been done to show the large experience which has warranted Dr Fayrer in publishing this work.

A great deal might have been done in the way of concentration, pruning, and arrangement, to make this book more valuable and more readable than it is. There is far too much repetition, too many utterly insignificant and useless details, and too little generalization.

With added leisure, Dr Fayrer will do well to extract the cream of what in his busy life he has only had time to record.

Annali di Chimica applicata alla Medicina. Compilati dal Dottor GIOVANNI POLLI. Milano: 1873.

THIS is a monthly periodical devoted to Medical Chemistry and Pharmacy. We have before us the number for last January, which should have been acknowledged sooner.

There is a good deal of information, judiciously collected from various sources, French, German, and English, as well as Italian, about the uses and preparations of different medicines. There is an article by Guiseppe Romei on the nature of the colouring matter of wine. The author claims to have separated two distinct colouring substances from red wine; but tells us little about their nature.

The longest article in the number, by Giovanni Battista Ayr, advocates a renewal of the ancient Greek and Roman practice of burning the dead. Naturally it is full of learned quotations, and the arguments of the author are seconded by the editor, who quotes from several letters of most excellent signores and most distinguished signoras recommending cremation as "a generous act towards the living in a profoundly poetical form."

On the Results of Thyrotomy for the Removal of Growths from the Larynx: Being a Reply to Mr Durham's paper in the Fifty-fifth volume of the Transactions of the Royal Medical and Chirurgical Society. By MORELL MACKENZIE, M.D. Lond., Physician to the Hospital for Diseases of the Throat, etc. Reprinted from the *British Medical Journal*, 26th April and 3d May 1873. London: J. and A. Churchill. Pp. 22; with Table of Cases.

STATISTICS, it is said, can, with proper management, be made to prove anything, and certainly no more amusing example of the truth of this axiom could be found than the one discussed in this paper. A limited number of cases are selected and examined by Mr Durham; from these he deduces the information that the operation is such a good one that an "earlier, bolder, and more ready resort to this method" is to be encouraged. From the same cases, with a few more of the same general character, Dr Morell Mackenzie deduces a quite different conclusion, and opposes the operation with much vigour and cleverness. Mr Durham, an active surgeon, approves of cutting into the thyroid from the outside, and removing the morbid growths with the knife. Dr Morell Mackenzie, a most dexterous laryngoscopist, prefers to remove the morbid growths *per vias naturales*, with the aid of the laryngoscope. It is quite natural that they should differ in opinion, but it is a great pity that they should get so hot on the subject. It is also unfortunate that Dr Mackenzie, not being a member of the Medico-Chirurgical Society, could not meet Mr Durham in discussion in the Society, instead of having to wait and have it out in a more solemn way in the columns of a weekly journal. As to the merits of the case, we agree with Durham that thyrotomy is lawful and advisable in cases where the growths from the larynx cannot be got rid of without it; but we agree with Mackenzie that removal *per vias naturales* is more scientific and less dangerous, and that thyrotomy has so many risks that

it should not be undertaken unless urgent symptoms are present. Let Durham open the larynx after Mackenzie has had a fair trial with the laryngoscope and forceps, and failed to cure the patient.

On Strictures of the Urethra: Results of Operations with the Dilating Urethrotome, with Cases. By F. N. OTIS, M.D., Clinical Professor of Genito-Urinary Diseases, College of Physicians and Surgeons, New York. Pp. 20. Reprinted from *New York Medical Journal* for March 1873.

IN this pamphlet the author draws attention to cases in which a chronic gleet discharge continues as the result of the slightest abnormal encroachment upon the calibre of the urethra, the urethra perhaps admitting with ease a No. 10 or 12 (English), and yet when tried by the bulbous sound, giving evidence of the existence of one or more contractions of its calibre. For the detection and complete relief of these, the author has invented an instrument which, constructed on the principle of a parallel ruler, puts these tight parts on the stretch, and the surgeon is thus enabled to divide them with accuracy and ease from within by a concealed blade. Three rather good cases are given in illustration of the good effects of this treatment; and the author makes the comforting and satisfactory announcement, that careful examinations of these with sound and endoscope, at an interval of from three to six months, show that no return of the disease had occurred.

A Treatise on the Pneumatic Aspiration of Morbid Fluids; a Medico-Chirurgical Method of Diagnosis and Treatment of Cysts and Abscesses of the Liver, Strangulated Hernia, Retention of Urine, Pericarditis, Pleurisy, Hydrarthrosis, etc. By Dr GEORGES DIEULAFOY, Gold-Medallist of the Hospital of Paris. 8vo. Pp. 394. London: Smith, Elder, and Co.: 1873.

FEW inventors have made such a rapid success in teaching the profession both how to use and how to appreciate their inventions, as Dr Dieulafoy has in bringing his aspirator into use. Not only is the name well known and the principle of its action approved, but a very considerable literature has already grown up around the subject. Many cases have been recorded, and even articles written in praise of the method, and no fewer than seventeen modifications or adaptations of the original instrument have been devised.

In this work the principle of the invention, its various forms and uses, are described, and numerous cases are given, in greater or less detail, illustrating its use and proving its value. Though the work

is evidently got up in a hurry, and is ill arranged, and contains many redundancies and repetitions, it conveys a great deal of interesting information, and thoroughly describes an instrument which is, we believe, a very great acquisition, both for purposes of diagnosis and treatment.

The writer of this has had one of Dr Dieulafoy's instruments in his possession for nearly a year, and is so convinced of its value as to put it to nearly daily use, so that he can, from his own experience, corroborate much of the author's praise of his instrument.

By aspiration is meant the union of the air-pump to make a *previous vacuum*, with a very fine capillary needle, which is connected to the air-pump by a tube provided with a stopcock. In the author's words: "The aspirator is prepared, or, in other words, the vacuum is made as a preliminary, and the needle is put in communication with the body of the pump, the stopcocks remaining closed. This hollow needle is then introduced into the region to be explored, and when it has passed the distance of a centimetre through the tissues (that is to say, when the opening at the point is no longer in contact with the external air), the corresponding stopcock of the aspirator is opened. What occurs? The air contained in the needle is at this point rarified by the previous vacuum in the body of the pump, and the needle possesses in its turn aspiratory power; it becomes an aspiratory needle, and carries the vacuum with it. Then, if pushed through the tissues, it is with the vacuum in our hands that we proceed to the discovery of the effusion. At the moment the aspiratory needle reaches the fluid, the latter rapidly traverses the glass index, gets into the aspirator, and the diagnosis is inscribed in the apparatus, without the interference of the operator. . . . In one word, we have in our hands an intelligent force, which has the advantage of being perfectly innocent." —Pp. 13, 14.

This one point about the "*previous vacuum*" really contains the gist of M. Dieulafoy's discovery, and distinguishes his plan from all previous methods of evacuating matter or exploring cavities. A good aspirator ought in itself to combine the advantages of a *previous vacuum*, of any amount of *subsequent vacuum*, such as is given by an ordinary syringe, and also act as a syphon. M. Dieulafoy's is useful in all these capacities.

Its value as an aid to diagnosis is very great, and on this point M. Dieulafoy enlarges with pardonable pride. He is also convinced of its harmlessness. "Punctures made by a needle which measures but half a millimetre in diameter are quite inoffensive. I have thrust these needles into almost every part of the body—into the joints, the liver, the spleen, the bladder, the intestines, the lungs, the meninges, and I can affirm, and a great number of persons affirm with me, that we have never seen consecutive accidents.—P. 21. Indeed, he ventures the following axiom: *It is, always possible,*

owing to aspiration, to search for a fluid collection, without any danger, whatever may be its seat or its nature.—P. 24.

We have not time to follow our author in detail through his argument; but, under the head of aspiration as a means of treatment, he gives the encouraging opinion that the frequent use of aspiration without any irritating injections often succeeds in drying up a fluid effusion at its source. The writer of this has had one or two cases which encourage him also in this belief. To be successful, the aspirations should be frequently repeated, and no attempt should be made to withdraw the whole of a large collection of fluid at one sitting, but rather by successive operations to allow the tissues gradually to contract and regain their normal relations. In cases where injections of cleansing or stimulating character are required, the aspirator is an exceedingly suitable instrument, as by simply opening and shutting the respective stopcocks it can at once be changed from an aspirator into an injecting syringe, and *vice versa*, so that the amount of the fluid injected can be exactly measured, and also the pathological fluid can be *gradually* changed into a curative one. The plan by which this may be done is described at some length at pp. 36 and 90, with considerable repetition.

That the author is sanguine and really believes in his instrument is seen in the following sentence, with which chapter iv. ends:—"In this summary I have only spoken of accomplished facts, but much yet remains to be done. Many other applications of the aspiratory method may be already foreseen. Why should the needle not be insinuated into pulmonary cavities of phthisical patients, to modify, by means of aspiration and injections, the walls of the cavity and the adjoining tissue? Why should we not perform direct bloodletting of the lungs and heart in desperate cases of congestion?"—P. 39.

The second part is on Aspiration in the Organs. 1. Of the liver. Hydatid cysts have been diagnosed and treated with success and without danger by this method. The exact procedure is described and contrasted with the older plan of the exploring needle. Several cases are given. One very curious symptom followed two of his cases, immediately after the operation—extensive urticaria appeared. In two other cases the same result has been found in the practice of other surgeons. Another, as yet unnoted symptom in such cases, M. Dieulafoy has seen twice, viz., the regurgitation (rather than vomiting) fatty matters after each meal. Abscess of the liver is also suitably treated by aspiration.

Retention of Urine treated by Aspiration.—From experiments on animals, and from now a large number of cases in man, it is now proved that in bad cases of retention, the urine can be easily, safely, and frequently removed by aspiration above the pubes. This chapter is eloquent and amusing, and the arguments perfectly incontrovertible. The method of operating to ensure safety is described shortly and simply. Aspiration may prove useful as a

means of injecting and washing out the *bladder*, and even the *stomach*. It is not available for *ovarian cysts*, except in the case of small unilocular ones. In *hydrocephalus* the results have been disappointing, but in *spina bifida* there has been some encouragement to persevere. *Hernia* has been a battle-ground, some saying that aspiration is harmless, and, by removing gas and serum, often saves the necessity for operation; others again say that bad effects have followed its use. Our own opinion is that, considering the safety and success of the operation for hernia when properly and in good time, aspiration should be used only in those cases who refuse the operation, or in whom the operation might present special elements of danger. In bad cases, where the hernia has been long down, aspiration might do much harm by allowing the reduction of bowel not in a state fit for reduction. By a large series of cases M. Dieulafoy contends for the harmlessness of the aspiratory puncture even in cases where it did no good. Many cases are given in which it seems to have aided reduction.

Affections of the chest, pleurisy, empyema, and pericardial effusions are all very suitable for treatment by the aspirator, and many illustrative cases are given. In dropsy of joints, and even in suppurating joints, the same method is applicable. We have not space to do more than recommend to our readers this book as a record of work of very great value. Haste in its publication, in these days of struggle for priority, may explain any repetitions and redundancies.

Observations on the Surgical Treatment of Ingrowing Toe-Nail. By GEORGE STILWELL, Surgeon, Epsom. With a Pamphlet by Dr COTTING, Boston. London: Churchill: 1873. Pp. 8.

THE eight pages of this unpretending little pamphlet contain: 1. Observations by author. 2. Description of operation by author. 3. Letter from Mr Maunder. 4. Infleshed toe-nail, a new operation for radical relief, by B. E. Cotting, M.D., Harvard. All these are on the same text, *i.e.*, to treat ingrowing or infleshed toe-nail; treat not the nail, as has hitherto been the custom, but the flesh around the nail. Find the edge of the nail with the probe, and then remove the whole of the granulations and hypertrophied cellular tissue on both sides if requisite. All the authors have a little tendency to exaggerate the pains and penalties of the ordinary operation, and have evidently been unfortunate in their experience of it, as we have not seen the recurrences and lamed feet they mention after evulsion of a portion of the nail, *if properly done*. However, the method proposed seems well worth a trial.

The Preventive Treatment of Calculous Disease, and the Use of Solvent Remedies. By Sir HENRY THOMPSON, F.R.C.S., etc. Pp. 72. London: J. and A. Churchill: 1873.

THIS little *brochure* consists of two lectures; one, on the Preventive Treatment of Calculous Disease, is simply extracted *verbatim* from a work lately reviewed in these pages by the same author, called *Diseases of the Urinary Organs*. The other, on the Treatment of Stone in the Bladder by Solvents, was also delivered as a lecture at University College Hospital, and is a very succinct and readable account of the various nostrums and devices which have been used for the cure of stone in the bladder without operative procedure. The discouraging result of all is, that only very small stones can be improved by solvents, and that only by a very prolonged process, while these are the very cases for which we have a rapid and certain means of cure in lithotripsy.

Part Third.

MEETINGS OF SOCIETIES.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXII.—MEETING XIV.

11th June 1873.—Dr SIMPSON, *Vice-President*, in the Chair.

I. *Dr Matthews Duncan* showed two microscopic preparations taken from a POLYPUS which he had lately removed from a patient, the account of which appears at page 97 of the August number of this Journal.

II. *Dr Young* exhibited a PORTABLE GALVANIC BATTERY.

III. *Dr Jamieson* exhibited TWO INCISOR TEETH, which were present at the child's birth. He removed them on the tenth day, as they were loose.

IV. *Dr Simpson* showed two MUCOUS CANALICULAR POLYPI, which he had removed by torsion.

V. THE USE OF THE ELECTRO-MAGNETIC CURRENT IN THE SECOND STAGE OF LABOUR. BY DR M'RAE, PENICUIK.

[This article appears at page 243 of the present number of the Journal.]

Dr Simpson had only seen galvanism employed once in obstetrics. It was in a case where premature labour was being induced, and

it certainly had the effect of starting the uterine contractions. He thought, if it was not necessary to apply the handles directly to the uterus, that the patient might as well hold them in her hands.

Dr Moir had never met with a case where he could not bring on labour pains. He had once tried galvanism in a case of post-partum hæmorrhage, and succeeded by that means in stopping the bleeding. He applied one handle inside the os, and the other over the fundus.

Dr Matthews Duncan had once seen it used in a case of hæmorrhage, but without any effect.

Dr Ritchie remarked that only one case had been reported to the Society during the last six years in which galvanism had been used in the second or third stage of labour: in that instance it succeeded.

Dr M'Rae replied, and stated that he had not had occasion to use it since 1869.

SESSION XXXII.—MEETING XV.

25th June 1873.—Dr THOMSON in the Chair.

I. UNUSUAL ILLNESS IN THE SEVENTH MONTH OF PREGNANCY. BY DR BALLANTYNE, DALKEITH.

[As no report of this case has been received, the discussion upon it is deleted.—Ed. *E. M. J.*]

II. THE COMPARATIVE ADVANTAGES OF FORCEPS, TURNING, AND PREMATURE LABOUR, ILLUSTRATED BY THE OBSTETRICAL HISTORIES OF TWO WOMEN. BY DR ANGUS MACDONALD.

[This paper appears in the present number, at page 214.]

Dr Matthews Duncan agreed in most points with the views expounded by Dr Macdonald. He would refer to the very unsatisfactory nature of recorded cases generally, in consequence of the unreliability of mere statements of pelvic measurements. There were many sources of error, especially want of experience, cases of considerable contraction being rare. He had opportunity occasionally of post-mortem verification of the errors made on this subject. But he had other very copious illustrations in his practice of such errors. Recently, he had a number of cases under his care where practitioners, judging from the difficulties of first labours, had diagnosed great contraction of the brim, and had recommended premature labour in subsequent confinements. In almost all of these he could find no good evidence of the contraction, and allowed the pregnancies to go to the full time; and the results of the labours showed that there was no notable contraction of the brim. It was well known that a prognosis from the history of a first labour,

unless carefully made, and on very distinct grounds, was very misleading. Every one who had extensive practice must have seen illustrations of this.

When we reflected on the slow progress of our knowledge of natural labour, which had nearly uniform conditions, and which was constantly going on before our eyes, we could not be astonished at the imperfection of our knowledge of difficult labours, whose conditions were never quite alike, and whose occurrence was comparatively rare. He would especially point out the great rarity, at least in Edinburgh, of the higher degree of contraction of the pelvis. Authors were particularly fond of discussing such cases; and there was so much writing about them, that one might think they were often met with, and were on that account urgent practical subjects. But he could from ample knowledge assert that, of cases of this kind—say, of two-inch conjugate diameter—it was uncommon for a practitioner to meet with one in a lifetime. Much, for example, had been written lately concerning the use of the cephalotribe, as if it were an instrument in frequent use in desperate circumstances. But he did not know of the occurrence in Edinburgh of a case for testing the highest powers of this instrument, or of the cranioclast, within the memory of man.

Cases of minor degree of contraction were not uncommon; but Dr Duncan was sure that the frequency of cases so severe as to demand operative interference was much exaggerated. He knew that in some quarters premature labour was frequently resorted to; and he could not divine what were the causes of its being required. He had already said that he had many opportunities of discommending the operation as unnecessary; and he felt encouraged by the results to continue such advice. In this course, even where there was pelvic contraction of moderate degree—say, down to near three inches—he was much encouraged by recent German writings, to which Dr Macdonald had referred. These writings, of eminent authors of large and carefully analyzed experience, said little for premature labour in this class of cases, a class in which there was considerable experience among us. Indeed, there had of late years arisen a passion for the frequent use of induced premature labour and for turning, which certainly was not based on good or sufficient grounds. It was notoriously the tendency of practitioners to suppose pelvic contractions to be greater than they really were; and there was a great attractiveness in operative interference. The great art of doing nothing is very seldom recognised as masterly inaction, being generally called the result of weakness, indecision, or deficiency in resource.

The rivalry between delivery by podalic version and by forceps was a very old one, and the struggle for preference in practice was now as keen as ever. But Dr D. regarded most of the combatants for either side as waging chiefly a vain war of words: the question

was a difficult one, not to be settled by views and ingenious arguments, but by observation, experiments, and statistics. Dr D. would be very cautious and reserved in expressing his opinion on this difficult subject. He indeed believed that there were cases in which the one mode of proceeding was preferable, and others in which the other was the better. This had been to a great extent recognised by German and Russian authors, and the criterion of preference was mainly the kind of pelvic contraction that existed. If, under these circumstances, he ventured to express a preference for the forceps and subsequent craniotomy if necessary, to delivery by podalic version and subsequent craniotomy if necessary, he must beg to be understood as speaking in a rough and very broad way. In his practice he had frequently resorted to both plans, and he would still do so. At present he did not wish to state the various categories of cases, and the proceeding best suited for each; he would only say that he regarded the forceps, gently used, as likely to be the safer practice in general both for mother and child.

There could be no difficulty in substantiating this preference in all cases when considerable delay would occur in the course of the artificial delivery. The advantages of delivery by podalic version were for the most part lost if the child were born dead, or permanently injured. If, after delivery of the body, there were delay in getting the head extracted, the child would likely be lost, whatever care might be taken. This circumstance led to great haste, and induced on both mother and child great risks. A very great objection, then, to delivery by turning, was the danger to the child of deliberate or slow proceeding, the danger to both mother and child of quick or hasty delivery. It was a grave mistake to suppose that more force could be applied to deliver by the feet of a child than by a forceps. It was a grave and common mistake to suppose that all was fortunate and successful if the child were born alive. Live birth was an invaluable and distinct test of a certain amount of success, but there were many dreadful ulterior results to be dreaded for living, and even for apparently healthy, infants.

Dr D. referred to the experiments of Joulin as indicating the proper course to pursue in advancing our mechanical knowledge of this mechanical subject, and pointed out the absurdity of the method by views and ingenuities. Among such were those generally referred to regarding the shape of the head, and its behaviour under pressure applied from below or from above, which were bad in method and erroneous in results.

Dr Pattison quite concurred in Dr Duncan's remarks.

Dr Gordon expressed great aversion to turning in cases of contracted pelvis, regarding the operation as highly dangerous to both mother and child.

Dr Milne was truly astonished to hear turning spoken of by more than one member of this Society as a dangerous and dreadful

operation. He had turned in numerous cases of contracted pelvis, with the best results; that was to say, he had saved many children that otherwise would have inevitably perished, and found no damage accruing to the mother. Indeed, in all the cases of version he had dealt with, he lost only one mother; but there there was great delay, and much injury done to the woman before he had been called in. He had reported that case as an instructive one in the *Lancet*. The induction of premature labour with version he viewed as an invaluable line of practice in such cases. To say, as had been done, that we had no cases of deformed pelvis in Edinburgh, was simply absurd. Though these were not, perhaps, very numerous, he and others, as was well known, had encountered them from time to time; and about the most satisfactory and cheering cases he had met with in obstetric practice were just those where, craniotomy or cephalotripsy having been performed in the case of the same woman more than once, living children were delivered subsequently by turning and artificial premature labour. He should discuss the whole subject at length at next meeting of the Society.

Part Fourth.

PERISCOPE.

HOMINES HIRSUTI.—*Apropos* of two hairy Russians, father and son, who have arrived for exhibition in Berlin, Professor Virchow delivered a lecture on the subject of hairy men to the Medical Society there. In this lecture Virchow pointed out that cases of this kind (hypertrichosis) could be comprised under three heads:—viz.,

1. Excessive hair formation of the male type occurring in females.
2. Hairy nævi, in which the hair growth is local, and occurs in connexion with other alterations of the skin.
3. The edentulous form, which is quite beyond the pale of what is known, and which probably can only have all its peculiarities effectually cleared up by a careful anatomical investigation. Cases of the first group are not unfrequent, are invariably female, and have their type in that Swiss woman depicted in the *Lancet* of April 1852, p. 421, who had gone to a London hospital to have her sex attested—a matter of the greatest ease, as she was four months pregnant. A vast number of such cases are upon record, reaching back quite to the Middle Ages. There is no history, and, so far as Virchow's knowledge goes, no evidence of anything the matter with the teeth of such persons, who only differ from their congeners in having a great development of hair on those parts in which it is ordi-

narily confined to the male. The second group is well known, and calls for no remark. To the third group belong the two Russians, now exhibiting in Berlin. The eldest is a man of over 55, Andrian by name, said to be the son of a Russian soldier from the district of Kostroma. He was born during the period of service of his reputed father, and has no resemblance to him, to his mother, or to a brother and sister whom he possesses. To escape the unkindness of his fellow-villagers, Andrian fled to the woods, where he lived in a cave, and was much given to drunkenness; even yet he is said to live chiefly on *sauerkraut* and *schnapps*, but his mental condition, which is truly none of the sharpest, does not seem to have suffered, and he is on the whole of a kindly disposition and affectionate to his son and those about him. Andrian was married, and had two children who died young; one of these was a girl resembling her father, but of the other, a boy, nothing can be ascertained. Fedor, the boy, exhibited with him, is three years old; and comes from the same village; he is said to be Andrian's son, born in concubinage; and it is most probable that this is the case, as it would be singular were two such creatures to originate independently in one small village. The peculiarity of these individuals is, that they have an excessive growth of hair upon one particular part of the body, namely, the face and neck; on the body and lower extremities there is also a stronger growth of hair, and particularly on the back and arms of the child there are sundry patches of 4-6 mm. in diameter, covered with soft yellowish white hair, 3-6 mm. long. Andrian himself has on his body isolated patches strewn, but not thickly, with hair 4-5 ctm. long. But all this is so trifling and subordinate compared with the hair growth on the face, that it is to that the attention is mainly directed. They are not therefore hairy men in the ordinary acceptation of the term, but more resemble some of the monkey tribe (the Diana monkey, *cuxio*, etc.). But one other peculiarity which they possess carries us yet lower in the scale of animal life: these hairy men are *edentulous*. Andrian has only the left eye-tooth in the upper jaw; Virchow has not stated how many teeth are in his lower jaw, but from the context it is improbable that he has more than his son, viz., four incisors. The son has no teeth in the upper jaw, hardly any alveolar process, and the upper lip very narrow, so that the upper jaw appears depressed; the father presenting the same appearance. It is somewhat singular that a similar family has long been known to exist at Ava, and was first described by Crawford in 1829, and since then by Beigel. Three generations of this family are now known to exist. The grandfather, Shwe-Maon, had a daughter, Maphoon, and she again a son; all of whom present precisely the same peculiarities as in the family of Andrian, not only as to the growth of hair, but also as to the teeth. The grandfather has in the upper jaw only four incisors, in the lower jaw four incisors and one eye-tooth, and these teeth did not appear till he

was twenty years of age. Maphoon has only four incisors in each jaw, the eye-teeth and molars are wholly wanting; the first two incisors appeared during her second year. The peculiarity of the hairiness in these individuals is of the same type as in Andrian and his son, in whom every part of the face and neck, usually only covered with lanugo, is covered with long hair, the very eyelids being so covered, the eyelashes being normal, while flowing locks come out of both nostrils, and also out of the meatus auditorius externus. At first sight the occurrence of two such families in two such distinct parts of the world seems to point them out as "missing links"—as the unreformed descendants of an earlier race of man. And our thoughts are carried back to the Ainos or hairy Kuriles, who are believed to be the remains of the aborigines of Japan, and who now inhabit the northern parts of the island of Jesso, and the southern part of the island of Saghalien. At first these aborigines were stated to be as hairy as our wild men; but from more accurate information, obtained by the Berlin Anthropological Society through the German resident, Herr V. Brandt, accompanied by numerous photographs and Japanese pictures of these Ainos, and from an examination of a skull recently obtained through Privy-councillor V. Pelican, Virchow is able to state with positiveness that, neither in respect of the formation of hair nor in regard to the teeth, have the Ainos any analogy with the Russian or Burmese hairy men. The Ainos are certainly hairier on the chest and extremities than the nations around them, but there is nothing peculiar in the distribution of the hair, and the males have hair only on the typical parts peculiar to man. There is not a shadow of a race-connexion between the Ainos and the Russian hairy men, and only the most prurient imagination could connect the latter with the Burmese family. No doubt, careful breeding could raise a new race of men from this accidental variety, just as various new races of domestic animals, dogs for instance, have been propagated from accidental varieties. Virchow, however, believes that the peculiarities belonging to the Russian as well as to the Burmese families depend upon peculiarities of innervation depending upon accidental congenital abnormalities in the trigeminus, within whose domain all these peculiarities present themselves only to be ascertained by careful dissection.—*Berliner Klinische Wochenschrift*, No. 29, 1873.

SALICIN IN OBSTINATE DIARRHŒA.—Dr I. B. Mattison of Chester, N. J., says, in the *Phil. Med. and Surg. Reporter* of February 1, that an assertion that the majority of practitioners, during an active professional life, meet with one or more cases of diarrhœa which prove utterly rebellious to ordinary treatment, will, we presume, pass unchallenged. After an experience limited to a few years, we have the record of several such instances, and the success in our hands attending the use of salicin has been so marked and gratifying, that we are induced to place it before the profession,

for the benefit of those who may not as yet have given this remedy a trial under similar circumstances. We administer in powder or pilular form, to children preferably the former, in any appropriate vehicle, in doses, under two years of age, of one half grain every four hours, and to adults after the following formulá:—℞ Salicin, 3j.; fiat pill. No. 24. Sig., Two pills every four hours. Its employment is followed after a short time by a decrease in the frequency of the evacuations, a return to their normal colour and consistence, and subsequent restoration to entire health.

PHOSPHORUS PILLS.—A writer in the *Druggists' Circular* gives the following formula for a pill of phosphorus, which he says can be made of small size, at short notice, and will keep without evolving fumes: Dissolve one grain of phosphorus in half a drachm of chloroform, and rub in a mortar with two scruples powdered liquorice root till all the chloroform has evaporated. Add half a drachm powdered soap, and work into a mass with water, and divide into twenty-four pills.—*Pharmacist*.

TRIPLEX PILL OF DR JOHN W. FRANCIS.—Take of powdered Socotrine aloes and powdered scammony, mercurial pill mass, āā 3j.; croton oil, m xx.; oil of caraway, fl3jss; tr. aloes and myrr, fl3ij. Mix the ingredients together by very thorough trituration, make a pill mass, and divide into four hundred pills. The usual aperient dose is one pill at bed-time till the natural condition is restored.—*Proc. American Pharm. Association*.

Part Fifth.

MEDICAL NEWS.

UNIVERSITY OF EDINBURGH—GRADUATION CEREMONIAL.—The public ceremony of graduation (chiefly in Medicine), in connexion with the University of Edinburgh, took place on Friday, 1st August, in the Assembly Hall. The Hall was completely filled upon the occasion. The Lord Justice-General, Chancellor of the University, presided, and was supported by Sir William Stirling Maxwell, Bart., Lord Rector; Principal Sir Alexander Grant, Bart.; and the various Professors. Professor Crawford having opened the proceedings with prayer,

Professor Turner said that, as promoter of the Medical Faculty, he had to present to his Lordship a large number of gentlemen who had been found qualified to receive degrees in Medicine. The following gentlemen were then duly presented:—

Degree of Doctor of Medicine under the New Statutes.

(Three stars indicate those who have obtained gold medals for their dissertations; two stars, those deemed worthy of competing for the dissertation prizes; and one star, those commended for their dissertations.)

*** Alexander Bennett, Scotland, M.B. and C.M., 1869; Thomas Marshall Bennett, England, M.B. and C.M., 1869; *** John Bishop, England, M.B. and C.M., 1870; George Bowman, England, M.B. and C.M., 1871; John Storrs Brookfield (B.A. New Brunswick), Nova Scotia, M.B. and C.M., 1871; *** William Macfie Campbell, Scotland, M.B. and C.M., 1869; * John Connell, M.A. St Andrews, Scotland, M.B. and C.M., 1867; John Archibald Cowan, Scotland, M.B. and C.M. (with second-class honours), 1870; Richard Wright Dodds, Berwick-on-Tweed, M.B. and C.M., 1870; Edward Liveing Fenn, England, M.B., 1865; James Gairdner, Scotland, M.B. and C.M., 1867; Peter Gowan (B. Sc. Edin.), Scotland, M.B. and C.M., 1871; William Bruce Gordon Hogg, Jamaica, M.B. and C.M., 1868; Alfred Hollis, Isle of Wight, M.B. and C.M., 1869; John Johnstone Salcomb Johnstone (B.A. Oxon.), England, M.B. and C.M., 1871; William John Kennedy, Scotland, M.B. and C.M. (with second-class honours), 1870; Henry Charrington Martin, England, M.B. and C.M., 1870; *** David Page, Scotland, M.B. and C.M. (with first-class honours), 1870; ** Arthur Perigal, Scotland, M.B. and C.M., 1869; James Turnbull Richardson, England, M.B. and C.M., 1870; John Smith, Scotland, M.B. and C.M., 1866; William Thomson, Scotland, M.B. and C.M., 1871.

Candidates who received the Degree under the Old Statutes.

Frederick Churchill, England, M.B. and C.M., 1867; William Frederick de Fabeck, England; Archibald Somerville, America.

Degrees of Bachelor of Medicine and Master in Surgery.

John Wilson Alston (B.A. Sydney), Australia (with first-class honours); Thomas Anderson, Scotland; William Andrew, England; Andrew Balfour, Hong Kong; Daniel Edgar Berryman, New Brunswick; William Bourke, Jamaica (with second-class honours); Charles Braid, England; John Milne Bramwell, Scotland; Wm. Arthur Brebner, Jamaica; John Brown, Scotland; David Herd Bruce (M.A. St And.), Scotland; James Kendal Burt, England; Alfred Midgley, Cash, England; Johannes Knockers van Oosterzee Cloete, Cape of Good Hope; Wm. Morriston Davies, England; Alexander Dumbreck, Scotland; Alfred Eddowes, England; Alexander Airth Edward, Scotland; Andrew James Elliot, Scotland; John Findlay, Scotland; Franklyn Stuart Fraser, Ceylon; Jean Anatole Galdemar, Mauritius; Hamilton George Gardner, India; Alfred Charles Godfray, Jersey; Frederick Le Maitre Grasett, Canada; Henry Alfred Chatham Gray, India; John Green, England; Robert Marcus Gunn (M.A. Edin.), Scotland; George Guthrie, Scotland; John Haggart (M.A. Edin.), Scotland; Charles Norton Hayman, England; Alexander Hodgkinson (B. Sc. Edin.), England (with second-class honours); Andrew Houston (B.A. Sydney), New South Wales; George Hoyle, England; Horatio Nelson Fairbairn Huggins, Trinidad; Thomas Hume, Scotland; Samuel M'Aulay Inkster, Shetland; Frederick William Jackson, England (with second-class honours); Edward Hughes Klien, Demerara; David Brown Logie, Orkney; James Charles Logie, Orkney; Richard Marsden Pilkington Low, England; John Greig M'Dowall, Scotland; John Lisle Hall M'Farlane, Jamaica; Charles Watson M'Gillivray, Scotland; George Hunter M'Kenzie, Scotland; Thomas MacQueen, Scotland; Roderick Macrae, Scotland; Reginald William Mapleton, England; John Meggat, Scotland; James Middleton, Scotland; William Birkmyre Miller (M.A. St And.), Scotland; Robert Davidson Murray, Scotland (with second-class honours); William Peel Nesbitt, England; Theobald Adrian Palm (M.A. Edin.), Ceylon; Charles Arundel Parker, England; William Thomas Pegus, Tasmania; William Preston, England; Frederick Parlett Fisher Ransom, England; Alexander William Watson Reid, England;

Henry Russell (B.A. MacGill), Canada; James Simpson, Scotland; Jas. Wilkie Smith, Scotland; Roger St Clair Steuart, Scotland; John Treharne, Wales; Andrew John Urquhart, Scotland; John Burnley Walker, England; Abraham Wallace, Scotland; George Arthur Warburton, Prince Edward Island.

Degree of Bachelor of Medicine.

John William Bramwell, England; Robert Laidlaw, Scotland; Murdoch Donald Macleod, Scotland; John Bisset Smith (M.A. Aberd.), Scotland.

Degree of Master in Surgery.

James Mitchell Monteith, Scotland M.B., 1872.

The Ettles Prize for 1873 of £40 to the most distinguished student of the year was awarded to John Wilson Alston, M.B., C.M.

Professor Fraser presented for the degree of M.A., Messrs James Miller Darling, Finlay Macrae, and James Cathcart White, who had not been able to appear and receive the honour on a previous occasion.

Bachelors of Science.—The following gentlemen received the degree of Bachelor of Science:—In the department of Natural Science, Alexander Hodgkinson, M.B., C.M. In the department of Engineering, John James and John Campbell Young.

Professor Turner then delivered the customary Graduation Address, which will appear in full in our next Number; after which the benediction was pronounced by Professor Crawford; and the newly-capped graduates having received the congratulations of the Senatus, the proceedings were brought to a close.

WE have much pleasure in bringing before our readers the following appeal. Many old students will be glad of an opportunity of giving their mite to commemorate their much-loved teacher, and they will have the additional satisfaction of giving it for a most worthy object:—

"Miller Memorial Home."—Shortly after the death of Professor James Miller, a few friends raised, by private subscriptions, about £1600 as a memorial to him. Knowing the late Professor's long connexion with and deep interest in Medical Missions, this sum was given towards the purchase of 56 George Square, as a "Home" for the students while being trained by the Edinburgh Medical Missionary Society as Medical Missionaries. This sum, however, was short of the purchase money by £700. Since getting the house, the directors have had to spend more than £300 additional, in necessary improvements and repairs. They now appeal, therefore, to the public for £1000, to enable them to clear off all debt upon the house, and to establish it as really the "Miller Memorial Home." Should more than the thousand pounds be received, it is proposed to place a suitable portrait or bust of the late Professor Miller in the "Home."

Subscriptions may be remitted to Dr A. G. Miller, 15 Shandwick Place, or to the Rev. John Lowe, F.R.C.S., 56 George Square, and will be acknowledged in future numbers of this Journal.

MEETING OF THE BRITISH MEDICAL ASSOCIATION.—The forty-first annual meeting of this Association was held at King's College, London, on Tuesday, 5th August, and the three succeeding days. This meeting has been a great success, not only as to the mere numbers present, but also as to the interest and importance of the addresses delivered, and the number of illustrious strangers present; amongst the latter were Professors Virchow and Langenbeck, and Dr Noel Gueneau de Mussy, etc. The inaugural address was delivered by the President, Sir William Fergusson, and consisted of a review of the medical and physical history of water from a sanitarian point of view. The address in Medicine, by Dr E. A. Parkes, was a most admirable and interesting retrospect of the progress of medicine during the last forty years, which well deserves to be read *in extenso* as a most valuable chapter of medical history. Forty years ago, the two great discoveries of the first quarter of this century—viz., Richard Bright's recognition of albuminuria, and Laennec's application of auscultation as a means of discovering the diseases of the heart and lungs—had hardly made their complete impression on medical practice. The urine was indeed tested in every hospital, and the stethoscope taught in every school, but enough of the old practice, and a sufficient number of its votaries, were still left to give the student of those days a pretty accurate idea of the chaos both in pathology and practice which prevailed previous to the employment of the modes of investigation devised by Bright and Laennec. Since then, Laennec's work has been improved and corrected, but little has been added to it; while Bright's albuminuria, like a newly discovered country, has been more accurately surveyed, mapped out, and divided into different provinces. The most important discovery during the last thirty years, in its influence on practical medicine, has been that of embolism and thrombosis, and for this we have mainly to thank Professor Virchow; its immediate effect upon diagnosis was great; its ultimate effect in promoting the recognition of the effects of minute capillary blockage, and of the transference of cells and their arrest in distant parts, giving rise to growths there, is even more important, and has hardly yet been fully recognised. Dr Parkes next referred to the discoveries in entozoic and microphytic diseases as exerting also a most important influence upon practical medicine, and then passed under review the most remarkable changes in the general doctrines of pathology during the last thirty years. During that time we have learned to recognise the great class of degenerations with its two divisions, those simply the result of age, and those which are the slow result of some slight but constant failure in nutrition. The great subject of inflammation came next under review, and Dr Parkes pointed out that, thirty years ago, the teaching of Rokitsansky, and the doctrine of "dyscrasia," were paramount, especially in Germany, but, since then, these have been replaced by the cellular pathology of Virchow,

which, in its turn, has been circumscribed by Cohnheim's (1867) rediscovery of the migration of the white blood-cells—a fact which has again given the old doctrine of exudation an empirical foundation. But the migration of the white blood-cells was first discovered twenty-five years ago (1839) by William Addison, now of Brighton, while its importance was duly appreciated by Charles Williams in his work on the "Principles of Medicine," and though neglected by others, all the microscopic phenomena of inflammation were nearly as well described by them as they are now. Dr Parkes next pointed out the changes in our views of tubercle since the days of Laennec, showing that already, thirty years ago, Thomas Addison of Guy's, John Elliotson, and Charles Williams, all asserted, in contradistinction to Laennec, the development of tubercle as the result of common inflammation. But it was from Reinhardt, in 1847, that Laennec's views received their deathblow, his researches having shown that there was no difference between tubercle and the products of common inflammation. Since that time we have obtained two important facts in addition to those of Reinhardt, the one in the implication of the lymphoid tissue in the most typical form of tubercle, and the other in the demonstration of the infective character of phthisical inflammation, for which we have to thank Villemin, Simon, Andrew Clark, Sanderson, and Wilson Fox. Dr Parkes then pointed out how this change of views as to the nature of tubercle had influenced medical practice, and then turned to the consideration of general fevers, showing how accurate our modern diagnosis was, where formerly all was chaos and confusion, and distinctly stating that if Dr Simon and his able band of investigators were allowed to guide the action of the State, it is not too much to say that these diseases, which in times past caused so great a mortality, and which even now kill so many persons, will become as much things of the past in England as leprosy and plague now are. Dr Parkes next traced the history of *morbid poisons* from the work of Robert Williams of St Thomas's thereon, which was published more than thirty years ago, down to the observations of Cose, Feltz, etc., and the germ theory of Lister. He next noticed the vast improvement in our diagnosis of diseases of the cerebro-spinal system, pointing out also how the laryngoscope, the ophthalmoscope, the microscope, the sphygmograph, and thermometer, which were nowadays in every one's hands, had improved our means of diagnosis and enlarged the dominion of certainty in medicine. Dr Parkes concluded his most able and interesting retrospect by an inquiry as to how far practical medicine had gained in utility by this increase of knowledge; and the conclusion he arrived at is, that amid the ceaseless activity of modern times medicine has bravely borne its part, and that the progress during the last forty years is full of hope "for the continued advance of our knowledge and for the progressive usefulness of our profession."

The address in Surgery was delivered by John Wood, Esq.,

F.R.S., Professor of Surgery in King's College, and is mainly taken up by a disquisition on antiseptic surgery, from which it seems that he thinks that antiseptics are not more useful than other dressings when the circumstances are favourable, while they are powerless to prevent the occurrence of unfavourable conditions, and that erysipelas may occur and abscesses become septic notwithstanding the most careful use of antiseptics. We leave him to the tender mercies of Mr Lister.

The address in Physiology was given by Dr Burdon-Sanderson with his usual ability, and was devoted to showing the intimate connexion between practical physiology and practical medicine, and first he pointed out certain matters in regard to the importance of which practical medicine and practical physiology seemed to be at variance, instancing in particular the results obtained by the Edinburgh Committee in regard to the action of mercury and its supposed influence on practice, showing that the conclusions which had been drawn from these results by some as to the inutility of mercury were no correct physiological inferences from the observations made, but merely an example of the undeserved obloquy cast upon science by practical men who attempt to apply its facts without a sufficient knowledge of their bearing; and he very correctly pointed out that it is clinical medicine which speculatively sets the questions which it is the object of experimental physiology to answer, while the danger to science begins when the practical man both sets his questions and gives his own answer in the name of science, and the danger to practice is when he allows such answers, falsely given, to lead him astray from the safe guidance of experience; and he illustrated the reciprocal influence of empirical speculation on the one hand, and of experimental scepticism on the other, by a reference to the physiology of the febrile state. In this he pointed out in a most lucid manner the present state of the question both from a medical and physiological point of view, showing that the two certainties we have to start from are, "First, that in the animal body heat is produced exclusively by chemical changes; secondly, that in the particular case to be investigated the rate at which the body is warmed is greater than can be accounted for by any mere accumulation;" the obvious conclusion being that febrile heat depends upon increased oxidation, the subject for inquiry being first, "When does this increased oxidation take place?" He then pointed out that of the three or four tissues which comprise the mass of the human frame, the muscular alone had been sufficiently investigated to enable us to say that the variations in the intensity of muscular oxidation are sufficient to account for all the variations in temperature which occur in fever; the conclusion from this being that "any process by which vital activity can be increased in a relatively large mass of living tissues is capable of producing a pyrexia which is in every respect, except its cause, a counterpart of that of fever." Hence we are brought back to the old definition of fever

as a reaction of the living organism against a stimulus—a definition, however, only of value as a finger-post, showing that we must seek for the proximate cause of fever among the “agents which act either directly or indirectly as excitors or irritants of living tissue.” Dr Sanderson concluded his most able address by an eloquent appeal to the Association to help forward pathological research as “a thing distinct from, though highly serviceable to, practical medicine and surgery.”

Besides these general addresses, each section was opened by a special address on its own special subject, and that of Obstetrics has—in the journal of the Association at least—the honour of two, one that ought to have been delivered by Dr Braxton Hicks, who, we regret to learn, was prevented by indisposition from doing so, and a second, which was delivered by Dr George H. Kidd. There was nothing of special importance in any of these addresses, though they were all very able, and those on public health novel. The Hastings gold medal was presented to Mr LAWSON TAIT of Birmingham, for his essay on Ovariectomy.

SANITARY SCIENCE GONE MAD.—Some few years ago we referred under this heading to a very scientific and clever Indian medical officer who, ignoring the dirt beneath his nose, referred the frightful mortality of the jail under his charge to a *jheel* or swamp twenty miles off. If the following were true, Edinburgh ought to be decimated by enteric fever, because probably nine-tenths of our milk is derived from cows fed on sewage grass. Mr Smee's cows must have been fed on sewage, not on sewage-grown grass. There is a vast difference between the two:—“Mr Alfred Smee, in a letter to the *Times*, gives the following explanation of the occurrence of typhoid fever from the use of milk:—We keep a small herd of cows at Wallington, from which my house in London is supplied with milk, cream, and butter. What is not required for our own use is sent to other members of the family, and when there is any surplus the neighbours purchase it. During this spring my son directed, without my knowledge, that the cows should be fed with a small proportion of sewage grass, when, without knowing the reason, the butter was so offensive that we could not bear it on the table. The other members of the family were loud in their complaints, and the neighbours for a long time came for no more butter. Upon inquiry, I heard of the use of sewage grass, which was immediately ordered to be discontinued, when the cream, milk, and butter resumed their former excellence. This seemed to me too seriously important to pass unnoticed, so I desired my son to repeat the experiment suddenly, without any notice, when the same results again occurred. The cows like the sewage grass, and the milk is slightly increased in quantity by its use. The milk has a slight rancid odour when about twenty-four hours old, and has this quality a day or two after the cows are fed with the grass. The butter becomes

bad about a day or two after it is made, and no care in its preparation can avert the rancidity. I have long known that the use of putrid manures affects the quality of vegetables, and have called special attention to the fact in the book of 'My Garden.' I did not, however, know till lately that the putrid matter could be taken by animals and communicated in the dangerous putrefactive state by the milk to other animals."

WE take the following from the *Scotsman* of 21st August:—"Much of the failure of the Dutch troops in the Acheen campaign has been due to deficiency in the medical department, and accordingly the Government has announced its willingness to employ British surgeons. The conditions under which our own young countrymen are invited to serve have just been communicated to me, and are as follows:—The applicant must lodge with the Chief of the Medical Staff of the Netherlands Army, at the Hague, his name and address, along with satisfactory references as to character. He must be under thirty-five years of age; he must be able to speak either Dutch, or French, or German well enough to be able to pass a short *viva voce* examination. His engagement will be for five years, and may be prolonged. Successful candidates will take rank with 1st Lieutenants on the staff; the pay will be 2250 guilders (about £185) per annum, with prospects of promotion. The Netherlands Government will grant a first-class passage to the seat of war, and a premium of 4000 guilders about (£330). The Batavian Republic has never forfeited the character given to it by Dugald Dalgety, according to whom 'its behaviour on pay-day might be an example to Europe.' Besides, the attractions of the Dutch Indies to the scientific surgeon (especially if he be a naturalist) are at once rich and unique."

THE life-size Anatomical Atlas of Paul Mascagni was the result of twenty-five years' labour on the part of that great anatomist, but its size was too great, and its price (2000 francs) too high for it to become so generally known as its merits deserved. The engraver, Antoine Serantoni, who executed the plates under the direction of Mascagni, has therefore determined to publish an edition of the work less unwieldy in size and more moderate in price. This will form one volume folio, comprising 25 parts, containing 150 plates, representing every part of the human body, one-third of the natural size, with explanatory text. Each part and each organ is twice figured—first, simply in outline, with letters of reference to the text; and, secondly, depicted as in nature, with their relations to surrounding parts. The price of the complete work is, with coloured plates, 600 francs; with plain plates, 300 francs—one-half ready money, the other half at three or four months' credit, on approved bills. Those paying the whole price in ready money to have 10 per cent. discount. Bills, etc., to be sent to Dr Alphonse Bos, 40 Rue Ricasoli, Florence, Italy.

LIST OF CANDIDATES FOR H.M.'s INDIAN MEDICAL SERVICE who were successful at the Competitive Examinations held at London in February, and at Netley in August 1873, after having passed through a course at the Army Medical School at Netley.

<i>Names.</i>	<i>Studied at</i>	<i>No. of Marks.</i>
1. Wilkie, D., ...	Glasgow and Berlin, ...	6142*
2. Battersby, W. E., ...	Dublin, ...	5745
3. Wall, A. J., ...	London, ...	5613
4. Moodie, R., ...	Edinburgh and Vienna, ...	5511
5. Goldsmith, S. T., ...	London, ...	5144
6. Thomas, A. A., ...	London, ...	4651
7. Tuohy, F. S., ...	Cork, ...	4616
8. Moynan, W. E. B., ...	Dublin and Galway, ...	4586
9. MacDonald, D. P., ...	Dublin, Cork, and London, ...	4413
10. Browne, W. R., ...	Dublin and Belfast, ...	4360
11. Baker, O., ...	London, ...	4343
12. Mallins, H., ...	Dublin, ...	3915
13. Wright, F. W., ...	Edinburgh, ...	3904
14. Robinson, M., ...	London, ...	3865
15. Leapingwell, A., ...	London, ...	3460
16. Dill, J. S., ...	Belfast and Dublin, ...	3200

* Has obtained the Herbert Prize.

LIST OF GENTLEMEN who Competed successfully for Appointments as Surgeons in H.M.'s Army Medical Service at the Examination held at the London University on the 11th August 1873.

<i>Names.</i>	<i>Marks.</i>	<i>Names.</i>	<i>Marks.</i>
1. Langridge, G. J., ...	2318	7. Thomsett, R. G., ...	1900
2. Fowler, B. W., ...	2219	8. M'Quaid, P. J., ...	1868
3. Webb, W. E., ...	2145	9. Tobbin, J. J., ...	1685
4. Gubbins, W. L., ...	2090	10. Spencer, F. H., ...	1660
5. Wood, O. G., ...	2005	11. Ring, J., ...	1640
6. Mapleton, R. W., ...	1905		

DR BELL PETTIGREW, F.R.S., will lecture next session on Physiology in place of Dr Arthur Gamgee, appointed to Owen's College, Manchester.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Medicine as a Profession: A Graduation Address, delivered 1st August 1873.* By WILLIAM TURNER, M.B. (Lond.), Professor of Medicine and Anatomy in the University of Edinburgh.

OWING to a severe domestic bereavement sustained by a much-esteemed colleague, the Professor of Surgery, whose duty it would have been, in the ordinary course of events, to have addressed you on this occasion, I have been called upon to occupy the place which he would so well have filled; and, as the representative of the Medical Faculty, to present you to the Chancellor for promotion to the dignity of Graduates in Medicine of our University, and to welcome you as members of our profession.

And here, at the termination of your state of pupilage, before the tie of teacher and pupil is actually severed, on the very threshold of your career as independent medical units, it may not be without profit to look around and consider in what direction you may find an outlet for your skill and energies; and in how far the academic training which you have received has provided you with weapons fitted to fight the great battle of life, with all its mingled reverses and successes.

Four or five years ago the majority of your number were just emerging from the as yet unformed (I do not say uninformed), but potentially active condition of mental life, which is the product of the education imparted in our higher schools, and in the literary faculties of our universities. To employ a biological method of comparison, you have, from that time to now, been passing through a process of differentiation, by which you have been gradually but surely moulded out of the plastic protoplasm, the resultant of a good general training, into that special development of professional life and activity which the title of graduate in medicine of our University, now bestowed upon you, is a guarantee that you have received.

For Von Baer's great anatomico-physiological law of the progress from the general to the special in development, is applicable

to the evolution of the complex framework of modern society out of the originally simple nomadic condition of the human race, just as to the differentiation of the various tissues of the body from the primordial cells of the embryo, or to the evolution of a complex organic form from its originally simple condition.

Most of you, I have little doubt, will, for at least the earlier part of your career, engage in the general practice of your profession, either by holding for a time junior offices in some public hospital or dispensary; or by attaching yourselves as assistants to some experienced practitioner; or, in the case of those who may already have acquired, by apprenticeship or otherwise, a sufficient amount of practical skill, by at once assuming the responsible position of independent practitioners.

Owing to the rapid growth in the population of Great Britain, more especially in the mining and manufacturing districts, and in the seaport towns; to the vacancies which, from various causes, occur in other localities at home; and to the demand for qualified medical men in our numerous colonies and foreign possessions,—a wide field for general practice is thrown open, and numbers of young graduates and licentiates from this and other schools of medicine are annually required for the performance of its manifold duties. And it is natural that it should be so. For the demand for the practitioner who can make himself generally useful in cases of injury and sickness, is of necessity much more widespread than can exist for the services of one who, with special qualifications, perhaps, in a more limited department, is fit to practise it and it only. This more extensive demand for the services of the general practitioner leads also to what many of you doubtless will regard as a very important consideration, a more speedy pecuniary return for the time and money which you and your parents have expended in the prosecution of your medical education. And although the incomes to be drawn from general practice fall as a rule below those which may in time be gained by the most successful specialists, yet they begin at a much earlier period of life, and without those long and weary years of waiting and uncertainty which have to be passed through by those who devoted themselves to the pursuit of a special line of practice.

But among so large a body of young graduates as I am now addressing, we may be sure that some, who may be so fortunate as to possess more ample pecuniary means, and to whom therefore the need of at once earning an income is not so pressing; or others who may feel conscious that they are more especially adapted for the prosecution either of a scientific or practical department of the profession, than for its general duties, will go through a still further process of differentiation. To some it may be an object of liking or ambition to practise medicine only, to others surgery, to others gynaecology, whilst to a limited number, perhaps, the study and treatment of mental diseases may prove attractive. For the suc-

cessful prosecution of one or other of these departments, not only will it be necessary that some additional years should be devoted to study, but that your attention should be more particularly concentrated on the department you have in view. For it should be remembered that your professional training has up to this time been general in its character. Your teachers, though themselves specialists in their own departments, have directed their energies to the development of their respective subjects so as to give to each of you the opportunity of obtaining a sound acquaintance with the various branches of your profession generally; and the several examinations to which you have been subjected in your progress towards the degree have had for their object to elicit if you possess, not a special proficiency in any one subject, but a solid foundation of general professional knowledge.

In carrying out a course of further study, it is above all things necessary that your period of hospital attendance should be prolonged. For the opportunities of subjecting to systematic examination such a varied series of cases as may make you adepts in diagnosis and methods of treatment, can most advantageously be obtained in the wards of an hospital. To some it may be more convenient to continue in attendance in this school; to others to migrate to hospitals in London or elsewhere; whilst others, again, may prefer to spend one or two years at the more important Continental universities. Your professional knowledge ought to make a rapid advance during this period of additional study. For, freed from the cares and worry of impending examinations, and already trained in accurate methods of diagnosis, your whole attention can be concentrated on particular departments of practice, and the value of different modes of treatment can be considered and compared.

To meet the manifold requirements of modern life and society, a small proportion of the members of our profession apply themselves to the special study of the diseases of some one particular organ, as the eye, the ear, the skin, the larynx, etc. In the larger Continental universities, more especially in Germany, these limitations of medical and surgical practice are more distinctly specialized than is the rule with ourselves; separate teachers devote their time and attention to these subjects, and consequently the instruction which is imparted is very complete and systematic. It is not to be wondered at, therefore, that those of our graduates who wish to qualify themselves for practice in these limited departments not unfrequently resort to one or other of the great Continental schools which has acquired a reputation from having attached to it one or more teachers of eminence in these departments.

The system of the Continental universities, especially in Germany, is more elastic than our own, and affords greater room for expansion in particular directions, so that the front which they present to the world, as regards both the number of teachers and the subjects taught, is much more imposing than anything we can

offer. This multiplication both of men and of subjects seems to depend upon two causes. Primarily, on the fact that the chairs are all endowed by the State with stipends sufficient to ensure to the professors a moderate competence. Their income is not made to depend, as with us, either altogether or in great part, on the fees of students; the same need does not exist, therefore, by discussing a subject in its more general relations, to attract large bodies of students, but the time and energies of the professors can be applied to the special cultivation and exposition of more limited departments.

But secondly, owing, in Germany, to the number of universities, to the double professoriate, ordinary and extraordinary, and to the system of *privat-docents*, the number of university appointments is much greater than in this country, their graduates are induced to devote themselves in a much larger proportion to the cultivation of literature and science, and the sum of their productions in the paths of discovery and original research—their intellectual output, if I may so say—is considerably in excess of ours.

Impediments of a not very easily surmountable character stand in the way of an extension of our staff of professors, not so much in the direction of instituting and endowing chairs for imparting instruction in subjects not falling within the ordinary course of professional training, as of giving a greater development to departments of already existing chairs, which, by the continuous growth of the sciences, have of late years assumed a special importance.

Unless sufficient endowments were provided—and of this there is no immediate prospect—an extension of the professorial system could only be carried out, either by exacting attendance on the newly-established chairs, or by permitting attendance on the new chairs to be substituted for that on some one or other of those already existing.

But to either of these propositions there are very serious objections. To load the curriculum by compulsory attendance on additional courses of lectures would burden the student, not only by demanding from him a greater expenditure in the way of fees, but by absorbing a considerably larger share of his time, sufficient of which is already spent in the lecture-room.

To substitute even a permissive attendance on a new chair for that on some one of those already existing, would be an injustice to the present race of professors. For the absence of any endowment at all of one of the chairs in the Medical Faculty, and the smallness of the stipends—scarcely worthy of the name of endowments—attached to the other chairs, render it necessary to guard against a diminution of the income which a professor, like any other man who devotes his time and labour to discharging the duties of a responsible office, is fairly entitled to, and, without the prospect of obtaining which, competent men would not be induced to prepare themselves for filling professorial positions. But, further, the chairs as at present constituted afford, each in its own depart-

ment, an opportunity to the student of making himself acquainted with the general principles of a subject; whilst, if he were allowed to substitute for attendance on one of these chairs a course from a new chair, in which only a portion of a subject were discussed, whilst the instruction in that portion might and probably would be much more minute, yet other portions of the same subject, of perhaps equal importance, would be altogether omitted, and the general training of the student would in so far be impaired.

Until, therefore, proper endowments, not only for new chairs, but for those already existing in the Faculties, be provided, either by private munificence, or by grants from the State, or by some special system of rating districts for the promotion of the higher education, such as has recently been suggested by my colleague, Professor Lorimer, we cannot, without inflicting hardships, either on students or professors, expect to have such an extension of the professorial system as prevails in the German universities.

But there is one method, which, without burdening the student with compulsory attendance on additional classes, or subtracting from the emoluments of existing chairs, might provide for an extension of our teaching system in those directions where development and growth seem most to be needed.

In a large school such as this, a year never passes over without one or more of our graduates displaying an ability to cultivate the more scientific departments of our profession, and a wish to devote himself, for a time at least, to the occupation of teaching. Scope for the desires of these gentlemen is in some instances obtained by acting as assistants to chairs in the University, or by acquiring appointments in colleges and schools elsewhere, or by becoming extra-academical teachers in this city.

Were the University to adopt a form of the *privat-docent* system, should it be found practicable to do so, then it would possess, in addition to its professorial staff, a proportion of young graduates, selected on account of having exhibited talents for research and aptitude for teaching, who would apply themselves to the elucidation and exposition of special departments of importance. In this manner not only would the University be enabled to offer encouragement to the cultivation of science, and retain within her walls those whose talents might do her good service, but our students, without compulsory attendance being exacted, would be provided with the opportunities, should they wish to avail themselves of it, of obtaining courses of instruction on special subjects, without having to go elsewhere for the purpose. The University would in this manner become a great training school, not only of students, but of teachers also.

I have as yet spoken only of private practice, as it presents itself either in its general or more special aspects, as an outlet for your energies, and a means of employing and utilizing the knowledge which you have acquired. But there are other directions in which

employment can be obtained that, to some of you, may offer a more congenial field for occupation and work. Some may desire to enter the public service of the nation as medical officers of the Army and Navy. To obtain appointments in either of these branches, a competitive examination must be undergone, and the selected candidates have then to spend some months at the great Government Hospital at Netley, where a special school has been organized for training medical officers in those subjects, methods of work, and duties which are most required in connexion with the services in question. At the expiration of this additional period of study, the candidates are subjected to another examination, and are then placed in order of merit.

It must be confessed that, amongst the graduates of this University, the same desire to enter these services is not now exhibited as was manifested at one time, even in the course of my experience. And the causes of this backwardness are, I think, not far to seek. The Chair of Military Surgery, which once existed in our University, prominently directed the attention of our students generally to the army medical service as a career to which they might devote themselves, and without doubt helped to draw to the University for purposes of study a proportion of young men who, from family or personal reasons, had, from the outset of their course, decided on pursuing this branch of the profession. By the abolition of this chair some years ago, this special feature of our school now no longer exists. But there are other powerful causes residing in the condition of the services themselves, which have diminished their attractiveness to our students: the moderate scale of payment attached to the junior appointments, the slowness of promotion, the routine character of the life in times of peace, the inadequacy of the retiring pension after years spent in foreign service, and with health broken, perhaps, by a residence in unhealthy climates, and the doubt which unfortunately prevails lest privileges and concessions granted one year will not, after the lapse of a few years, be again withdrawn. And although, in more stirring and warlike times, when the pulse of the whole nation throbs at the recital of deeds of daring, when the love of adventure, which acts as a powerful incentive to so many men, is aroused, and when opportunities for acquiring personal distinction can be obtained, the services may regain their popularity with our students; it must be confessed, that in these days of peace, when men look at things in a calmer and more practical way, and are disposed to follow that line of life which is likely to prove most lucrative, the inducements to enter the services are not so powerful as to outweigh those which are offered by even a moderate amount of success in private practice.

The ever-increasing attention which has, for some years past, been paid to the circumstances affecting the health of the people, the necessity which has gradually forced itself upon the public mind of counteracting, by means of legislation, some at least of

those injurious influences which, either through private greed or municipal neglect, have slowly but surely, more especially in our large towns, been shortening the duration of human life, and deteriorating the stamina of our race, led to the passage through Parliament last year of an Act, which has virtually opened up a new and most important branch of public service to the members of our profession.

In section 10 of the Public Health Act, 1872, it is enacted that it shall be the duty of every urban and of every rural sanitary authority, throughout England and Wales, to appoint from time to time a legally qualified medical officer or officers of health, for the efficient execution of the purposes of the Sanitary Acts.

To this officer is entrusted the duty of ascertaining, by systematic inspection, the influences which affect injuriously the public health within his district. He must inquire into the various local circumstances, from processes of trade or otherwise, which may contaminate the water, or foul the atmosphere, or affect the drainage of the district. His attention must be directed to the ventilation of houses and workshops, so as to guard against overcrowding. He must pronounce, if called upon, that articles of food exposed for sale are wholesome and fit for consumption; and in the case of the outbreak of epidemic disease, he is to advise as to the measures to be taken to prevent its extension.

In order that duties involving so much responsibility should be efficiently performed, something more than an ordinary medical training is necessary. A well-instructed health officer should have a knowledge of chemical processes and modes of work more minute than is usually possessed by the practitioner. He should be able to work with the microscope, and be capable of recognising the appearance and character of at least all the more common objects which have to be examined by the aid of that instrument. He should possess a knowledge of the principles and methods of construction of houses, workshops, and public buildings, with a view to their proper ventilation; and he should have as much acquaintance with engineering and geology as will enable him to advise in the proper laying out of streets, and in the arrangement of sewers, drains, and other means of getting rid of refuse.

But besides all these professional qualifications, others of a more personal character are not unnecessary. He must be precise and systematic in his habits, and possess tact, temper, and judgment. For whilst he should be prepared to advise the sanitary authorities on all matters affecting the health of the district, and require from manufacturers, and other employers of labour, a strict attention to such precautions as may be necessary to guard against what may prove injurious to the health either of the persons employed, or the inhabitants of the neighbourhood; yet he should not recommend needless and vexatious restrictions, such as may check trade, and perhaps drive it out of the district.

Although the need which suddenly arose under the provisions of the Act for the appointment, within a limited period, of a large number of medical officers of health throughout England and Wales necessarily led to the selection from the general body of the profession of the present officials, there can be little doubt that in course of time this department of the profession will require to be organized on a distinct footing, and that those who aspire to practise it will have to undergo, in addition to the usual medical education, a special preparation and training.

When the State imposed duties of so onerous a nature on the members of our profession, it might have been expected that it would at the same time have undertaken to provide those who might be selected to act as medical officers of health with the means of enabling them to obtain that special education which is necessary for the efficient discharge of these duties. I do not refer to an actual payment of salary to the health officers during the time occupied in their special training—although that would not have been without precedent, as the army medical probationers at Netley receive certain allowances during their time of training in that school—but to arrangements for furnishing them with the means of instruction. From a Government which has shown no particular desire to further the advancement of science, and which has thrown frequent discouragement on efforts to obtain grants of public money for the promotion of the higher and more specialized kinds of education, the establishment of a distinct training school for the public health service, like the Netley school for the army and navy medical service, could scarcely have been looked for. But something ought to have been done to encourage and promote the expansion and utilization of the resources of the existing educational bodies which are willing to undertake to provide the necessary instruction. In our own University, for example, where the laws of public health form an essential part of the teaching of the Chair of Medical Jurisprudence, and where our Professor is not only willing, but most anxious to develop still further this most important subdivision of his course, the sheer want of class-room and laboratory accommodation stands in the way of organizing such a system of instruction as would prove a credit to the University, and fill a growing public want. A grant of money from the Treasury would enable the University to supply this want, and to send out each year graduates trained to perform the duties required of the public health medical officers.

To the credit of the sanitary authorities in many districts, both urban and rural, salaries have been conferred on the medical officers of health sufficient to secure their entire time and services, and to show that the authorities are desirous of working the Sanitary Acts in an efficient manner. We are glad to see amongst the number of those who are being promoted this day from the degree of Bachelor to that of Doctor of Medicine, a gentleman, Dr David Page, who

was appointed a short time ago medical officer of health for the county of Westmoreland. But in some districts the authorities, who in these instances are miscalled sanitary, by attaching to the office a minimum salary, have indicated, if the work intended to be done by the health officer is to be estimated by the amount of money to be paid for doing it, that the policy of letting things alone is to be pursued, and that the requirements of the Acts are as far as possible to be evaded.

Up to this time, the University has not been in a position to offer to the most distinguished of her young medical graduates rewards of pecuniary value to tempt or enable them to devote one or more years to further study. Except the Ettles Prize of £40, which the Medical Faculty has adjudged this year to Mr John Wilson Alston, M.B., no money prize has been open to competition amongst you. But at the graduation next year, an important and valuable Surgical Fellowship, established to commemorate the long and eminent services of our late colleague, Professor Syme, and for the successful foundation of which we are indebted to the energetic action of Dr Charles Murchison, will for the first time become available. And it may be well to remind you that some at least of your number have an interest in this fellowship. For by the terms of the deed of foundation, "The fellowship shall be open to competition among Bachelors of Medicine of not more than three years' standing at the time of the competition, who shall, in the opinion of the University examiners, have passed a creditable examination for the said degree. The fellowship shall be awarded to the competitor who shall present the best thesis on a Surgical subject, giving evidence of original research or practical talent, on his appearing as a candidate for the degree of Doctor of Medicine; but it shall be competent for any qualified person who does not desire to proceed to the degree of Doctor of Medicine to present a thesis, for the purpose of competing for the fellowship."

Further, I may direct your attention to the Prize of £60, which in course of time we have reason to hope will become a fellowship, established to commemorate the high scientific attainments and distinguished University career of the late Professor Goodsir. The first award of this prize will be made in 1875. Like the Syme Fellowship, it is not restricted to the graduates of the year in which it is bestowed, but is open to all graduates in Medicine of the University of not more than three years' standing. Graduates of this year, therefore, are entitled to compete. But this prize is also open to licentiates of the Royal Colleges of Physicians and Surgeons of Edinburgh of a like standing, who have attended within the University the same number of classes as are required for the degree of M.B. As the prize is founded with the object of encouraging original inquiry in those departments of medical science which Professor Goodsir himself so successfully cultivated, it will be awarded to the author of the best essay containing results of original investigations in Human and Comparative Anatomy,

both normal and pathological, or in Experimental Physiology. The candidates may select any subject in these departments which they may feel themselves best fitted to inquire into; and I trust that some of your number will be found in the list of candidates.

It has long been a subject of regret, that we have not had in the Medical Faculty the means of offering some pecuniary reward, in the form of Scholarships or Bursaries, to deserving students in the earlier years of medical study to aid and encourage them in their career. In the course of my experience, I have too often seen able young medical students of limited means compelled to devote no small share of their days, and nights also, to the drudgery of teaching schoolboys the rudiments of classics and mathematics, or to other extraneous pursuits, as a means of earning money sufficient to enable them to prosecute their medical course. The time and energy employed for this purpose had necessarily to be subtracted from what would otherwise have been devoted to the subjects of professional study; and in so far, by diminishing the force applied to their proper work, rendered more difficult the acquisition of that professional knowledge and position, which some of the gentlemen I refer to have so worthily attained. With a well-organized bursary system, this waste of power would be avoided. The able medical student of limited income, by acquiring a bursary, would find it unnecessary to eke out his means of subsistence by the drudgery of teaching subjects foreign to his present line of work; and by giving up his whole time and energies to his profession, both he and it would be the gainers.

It gives me great pleasure to state that, in the forthcoming session, a beginning will be made towards attaining this much desired object. Through the liberality of the late James Grierson, Esq., of Ogs Castle, and from other sources, the University will be able to offer annually three bursaries for competition amongst medical students: one Grierson bursary of £20 a year, tenable for three years; another of the same value, tenable for two years, a preference being given, according to the terms of Mr Grierson's trust-deed, to young men natives of the parishes of Crawford and Leadhills; and a Tyndall-Bruce bursary of the value of £25, tenable for one year, to which no restriction as regards place of birth is attached.

If time had permitted, I would have taken this opportunity of directing attention to other wants of the University, more especially the urgent need that exists for more and larger class-rooms, for a new museum, for laboratories, and for rooms in which an efficient system of practical instruction in all the departments of medical science may be carried on. To put this school of medicine on a level, not only with other universities, but with newer institutions springing up in several large cities, to enable us to keep our place in the great education race, it is essential that these should be provided, and that without delay.

That our University should not lose its position as the largest school of medicine in the British Empire, as the institution in which a thorough medical education can be imparted, is a matter which interests not only us who are more immediately concerned in her teaching, but the citizens of Edinburgh generally. For not to speak of the advantages which accrue to the city from being a great educational centre, attracting young men and families from all parts of the world for purposes of study, it is surely of the utmost importance to the inhabitants themselves, to have at their own doors an institution capable of providing their sons with a complete scientific and practical professional training.

The profession of Medicine, Gentlemen, which you have chosen, and as members of which you have now been enrolled, offers a wide field for work and usefulness. Unlike the Church, you are not restricted to a belief in a particular creed. Unlike the Law, your practice is not limited to a single nation or district. You can pursue your career in any country or clime, where sickness is to be combated, where the preventable causes of disease and death are to be detected, and where relief is to be afforded to suffering humanity.

In wishing you God-speed in your life-journey, let me exhort you ever to keep in mind that, as Graduates of this University, your profession is to be regarded not merely as a means of making an income, and of securing a respectable or honoured position in society, but as a high and noble calling, by the exercise of which in a wise, patient, and loving spirit, you may promote the progress of medical science, and confer inestimable benefits, not only on individuals, but on the human race.

ARTICLE II.—*On the Causes of the Predisposition to Phthisis among the Natives of the Hebrides and West Highlands.* By K. N. MACDONALD, M.D., etc.

IN the number of the Edinburgh Medical Journal for June, I observe an article, by Mr Robert Smith, on the above subject, in which he endeavours to show that the causes of the predisposition to phthisis, which is said to exist among a portion of the inhabitants of the North-west Highlands of Scotland, are more or less peculiar to that people, and entirely under the control of man.

As, however, to my mind, the several causes enumerated are in themselves not sufficiently clear in the way of proof, I purpose making a few observations on the same subject, with a view to testing the accuracy of Mr Smith's statements. In doing so, I shall endeavour to stick to observed facts on the one hand, and avoid national prejudices on the other.

Mr Smith begins by saying that "the immunity from tubercular

consumption which has been observed in the Western Isles, and in portions of the North-west Highlands of Scotland, is not uniform throughout the district. It varies in degree from an exemption from the disease which is almost complete, to one which it seems difficult to establish." Now, in my opinion, a serious omission occurs here, to start with, inasmuch as that neither the portions of country where "the immunity" exists, nor where the "latent tendency" prevails, have been specified.

"Side by side, however, with this immunity from phthisis," there is a "widespread latent tendency to the disease among the people, as proved by the many deaths in childhood from scrofulous ailments, and by the remarkable fact, that the natives of these districts readily contract the complaint when they leave their native land for the south." Here is a very good specimen of what I consider a preconceived notion acting upon the imagination, where something must be made out, because somebody has observed—probably upon rather flimsy evidence—that Highlanders, under certain circumstances, are liable to consumption like other people, *ergo*, a "latent tendency" to the disease must exist among them, which, strange to say, manifests itself chiefly when the native migrates to more favoured and sunny climes! At the same time, nobody knows anything about this "latent tendency," except that it remains dormant until the Highlander leaves his own country, and then he falls a victim to a disease that was either hereditary, or acquired in an undeveloped state, but which might never have troubled him if he had remained at home. I, for one, do not understand this, and I should like to meet with anybody else who does; for, in my opinion, if this "latent tendency" theory of Mr Smith's be true, it does not much matter under what circumstances the Highlander may live after leaving his own country, so far as his proclivity to this disease is concerned. And it is difficult to understand how such a peculiarity of constitution should exist under the influences of a cold, changeable, and damp climate.

Notwithstanding, however, the beneficial effects of climate, as estimated by Mr Smith, in a sanitary point of view, he goes on to say that "when men are subject to long exposure, to drenching rains," etc., and "compelled to walk in wet clothes," and "struggling against a fierce wind, it is not to be wondered at if they become exhausted, and the seeds of consumption are laid in the system: or, if they escape that danger, the constitution is permanently injured and weakened, and the succeeding generation inherits the weakness from their fathers."

As, however, the "native population need not ordinarily expose themselves thus, for, having no settled occupation, they are able to remain within doors during seasons of excessive storm," I do not see how this cause bears upon the subject, especially since it is "the shepherds" that suffer chiefly in this way, who, "as a rule, are *not* natives." Even if they were all natives, it would not affect

the question much, for it must be borne in mind that they only form a very small percentage of the population, and, "as a rule," they are a vigorous class of men, who beget their children when they are comparatively young and strong; and, so far as I am able to judge from a considerable experience, and personal knowledge of their condition and habits, I am unable to recall to memory more than one instance of phthisis brought on through overwork and excessive exposure. On the contrary, I have frequently observed that the children of shepherds and cowherds are fully as robust as those of the ordinary crofters and fishermen, because they generally possess one or two cows, and often have no rents to pay, consequently are not so pressed for the means of subsistence. Besides, the "drenching rains," and mists, and "fierce winds," are not regarded as at all formidable by those who are exposed to them, for they are generally accompanied by soft westerly and south-westerly gales of wind, which materially mitigate their effects.

I have been repeatedly assured by native shepherds, and others who had been exposed to the elements more or less for twenty years and upwards, that frequent drenchings do them little or no harm; and I have hundreds of times seen individuals of this class dry their soaking clothes by standing opposite a fire, neither from laziness nor from not having a change of clothes, but from the conviction that the rain did them no harm; and, undoubtedly, the principal disease from which this class suffers in old age is chronic rheumatism.

Mr Smith's description of the "cottage of the Highlander" is very fair, only that it applies to the worst class of cottage. The "four walls" he alludes to, I presume, consist of two walls to form the sides of the cottage, and two walls to form the ends, which are, of course, necessary to form the skeleton of an edifice. "The hole in the roof," which is simply a creel open at both ends, and fastened in such a way as to give free exit to the smoke, ought to create a considerable current of air, as the doors are rudely constructed and badly fastened, and, according to the description before us, the "crevices between the stones of the walls" not being even filled up with mud.

Notwithstanding this, however, "the whole atmosphere of the place is redolent of damp, peat-reek, and unwholesome effluvia."

The feelings of a stranger unaccustomed to the smell of peat-reek can readily be imagined on entering one of these dwellings; but it remains to be seen whether matters are actually so bad as alleged.

In the first place, it must be observed, that if the "hole in the roof," which acts the part of a chimney, is not closed every night, the air that gets in through the "crevices in the walls" will create a sufficient current of air to keep the sleeping apartments in a healthy state. Moreover, the mere fact of a cow or a pet sheep being in the opposite end of the house, with half-a-dozen hens upon a rafter, does not appear to me sufficient to warrant one in coming

to the conclusion, that there is a diminished quantity of fresh air circulating in these dwellings; for, be it remembered, the night-soil is never allowed to accumulate under such circumstances. It must have been a very extreme case where ducks were seen under a bed, and, to say the least, it was a very unnatural place for them to be in, for of all domestic fowls, they are the least likely to seek shelter in stormy or wet weather. And, in the second place, it does not necessarily follow that the sallow complexions and withered frames of those who are "compelled" to remain within doors should be caused by the state of the atmosphere they breathe. On the contrary, as these appearances are generally accompanied by old age, it merely shows that they must have lived under more or less favourable circumstances, by which they attained to that period of human life when the vital powers show symptoms of decay. To my mind, therefore, it does not appear at all clear that the question of breathing noxious effluvia and diminished supply of fresh air by the inhabitants of these dwellings has been at all properly established. If it were, one would naturally expect a greater prevalence of fevers of a low type among them than is actually to be found. Besides, it is a well-known fact in the Highlands, that dairymaids who sleep in byres, quite close to perhaps half-a-dozen cattle, are more robust than most other women; and this can be accounted for by the fact of their being in the habit of drinking considerable quantities of fresh milk. At any rate, noxious effluvia do not appear to have much influence upon their rosy cheeks.

As to the question, "how far carbonic acid gas breathed in excess," and combining with salts of lime in the system, may tend to the "direct deposition of tubercle," I am not prepared to decide further than that I have no doubt it will, like many other questions of a similar nature, die a natural death.

With reference to personal cleanliness, I need only remark that the Highlanders do not appear to differ much from the inhabitants of other cold climates in the way of being averse to the use of daily ablutions, especially in winter, for frequent bathing, at this season of the year, is not absolutely necessary, so long as under-garments are changed pretty often; and, I have no doubt, instances might be found of men who do not bathe half-a-dozen times in the year. I myself knew an instance of a man—still living—who told me some years ago, that he had not bathed for forty years, and, strange to say, he has never been troubled either with chest complaints, or skin or kidney diseases! What would hydropathists say to this? As an instance of the supposed great importance of daily ablutions, I may observe that the natives of India are bound to bathe daily, and they are both a short-lived and dirty lot withal. In forming an opinion of the habits of a people, the effects of climate ought always to be taken into consideration, for it has exercised a greater influence upon the human race than any other circumstance, i.e., in the widest sense of the term climate.

Mr Smith, however, is right when he says that the supply of food to the inhabitants "is far from regular," and he is even below the mark in stating that some of the poorer people live for several weeks in succession upon potatoes; and this seems to me, more than any other cause, to have a prejudicial effect upon the health of those who are constantly struggling to procure the necessary means of subsistence. Poverty of itself always engenders more or less depression, but I am not aware that the Highlanders are either a "peculiarly nervous people"—unless in the sense of being strong-nerved—nor melancholy, nor possessed of minds "ill fitted" to be thrown upon their own resources. This latter allegation, their pride would never allow them to admit, and I doubt very much if it can apply to a proud, haughty, and courageous race, who feel a conscious superiority of mental and bodily vigour; and I am sure that every one who has witnessed their reel-dancing, especially for the first time, must have been impressed with the amount of latent exuberance of spirits exhibited, notwithstanding their outward reserve towards strangers.

We now come to the questions of the effects of intermarriage and hereditary predisposition. As to the former, I believe that its influence—so far as these people are concerned—has been very much exaggerated, although common observation tends to establish that there is a great deal of truth in the latter. The general inference drawn from the effects of close blood-relationship is, that it causes a deterioration in the "physique" of a people. Now, this cannot be held to have had much influence upon the Highlanders in former times, and it operates less now than ever it did before; for, since the last insurrection of 1745, there has been a gradual subsidence of all political influences that might tend to prevent their mixing more or less with their southern neighbours, and all that remains of ancient clan feuds is to be found only in their songs.

Even supposing that southern influence had never been brought to bear upon them, the predominance of one or two clans would eventually have absorbed all the rest.

It is said that the "physical features of the land *everywhere* offer natural impediments to the existence of communication between its several parts," and that the numerous arms of the sea "complete the isolation."

It is all very well to make use of beautiful phrases like the "mountain wilds and fastnesses of the north," and to talk of the "mountain barriers" that "cut off the Celtic people from easy communication with their southern neighbours," etc., which sounds like as if the people lived on the tops of the mountains, whereas they have always occupied the most fertile glens and valleys. But a single glance at the face of the country must make it apparent to every intelligent man that the natural impediments to entering the most remote portions of the Highlands are almost *nil*, when it is remembered that, for thousands of ages, the present glens of Perth-

shire, Argyllshire, Aberdeenshire, and the plains of Nairn and Moray shires, have presented the same physical aspect as at the present day, with the exception, of course, of roads and railways, and probably a greater extent of country covered over with a stunted and crooked jungle. And though it is true that in former times the several passes were jealously guarded against the inroads of invaders, it is also true that wife-stealing from the Lowlands was a very common practice, which, together with the Scandinavian element that "from time to time" was brought to bear upon them, a greater infusion of fresh blood must have found its way into the Highlands than superficial observers would be ready to admit.

I need only say further, that many of the impressions that have found their way into the minds of writers upon Highland matters, are as erroneous as they are absurd. Any one who has lived among the people must see at a glance that it is just as rare to find "clans hostile to each other," inhabiting "opposite shores of the same loch," as it is usual in Mr Smith's opinion. And as to the assertion that "each man, woman, and child" in the several "townships" is related to the rest, I believe to be equally fabulous.

Another fact that makes me think that marriages of consanguinity could have had little effect upon the physical condition of the Highlanders, is, that they do not appear to have much deteriorated, at all events within historic times; and if the flattering accounts of the achievements they have attained on the battle-field are to be trusted—and the best men by no means join the army—they are still second to none in courage, physical strength, and powers of endurance, which nothing but stout limbs and good lungs could account for.

I have yet one more "bone to pick" with Mr Smith in reference to the prevalence of scrofulous diseases in childhood. Though I do not believe that there is a complete immunity either from scrofula or phthisis in any portion of the Highlands or Western Islands, I must say that during the four years I practised, both in Skye and the mainland, I could not aver that the death-rate from scrofulous diseases among children was at all striking. On the contrary, I am of opinion that this was a rare cause of death *per se*, and I had excellent opportunities of coming across them, so far as an extensive rural practice is capable of bringing one in contact with the diseases most prevalent among a people. Scrofula and consumption are not at all uncommon in Skye and the adjacent islands, and due, I believe, entirely to deficient aliment, for wherever this is supplied in abundance, the people are remarkably strong and healthy. The common belief among the more intelligent classes is that scrofula and insanity have some connexion with syphilis, and those who are unfortunate enough to have any relations in whom either of these diseases can be traced, are supposed to have bad blood in them, which is always considered by parents before allowing their children to contract new relationships that might bring disaster upon an unborn generation.

Now, in conclusion, I must observe that there can be no doubt many of the causes which oppress the Highlanders, both physically and mentally, are "strictly and entirely under the control of man," but it is a different question whether said "man" will remove them, or make any tangible efforts towards so laudable an object, at all events in our generation. And it would be well if those who concern themselves with Highland affairs were to consider that the accounts of historians should always be taken *cum grano salis*, and that they themselves would not be much different under similar circumstances. Anglo-Saxons are under the impression that the Celts (Kelts) are different from all other races, possessing peculiarities of their own, in which philosophers, even like Charles Darwin, concur, when he quotes the following from Mr Greg:—"Given a land originally peopled by a thousand Saxons and a thousand Celts—and in a dozen generations five-sixths of the population would be Celts, but five-sixths of the property, of the power, of the intellect, would belong to the one-sixth of Saxons that remained." Is it not strange that the idea never entered the minds of either of these philosophers, that if the case quoted were possible, the "one-sixth Saxons that remained" would be much more likely to have broken heads than handfuls of property, etc., if, indeed, they were not altogether exterminated? Though the above quotation evidently refers to the Irish, I have introduced it here to exhibit its absurdity.

Though my opinions differ very widely from the conclusions at which Mr Smith has arrived, it is quite possible that further investigation may induce us to alter our opinions; but until trustworthy statistics are produced, I consider it necessary, in the interests of truth, to exercise a wholesome scepticism upon all questions that have a scientific bearing; and it would be well if all impartial observers were more careful in their deductions; there would be less reason for letting their imaginations, like those of the disciples of Aristophanes, "soar like a bird" into the unknown regions of space.

ARTICLE III.—*Bone Absorption by means of Giant Cells.* By
ALEXANDER MORISON, M.B., C.M., Ed.

DURING the summer of 1873, I studied the development and absorption of bone, in Professor Kölliker's laboratory in the Anatomical and Physiological Institute of the University of Würzburg. There I chiefly collected the material for this paper. From Professor Kölliker, who kindly put at my disposal a large number of his own preparations illustrating this subject, I received every assistance. In a paper read before the Würzburger Physikal-Medicin-Gesellschaft, Kölliker showed that a typical absorption of bone tissue occurred at many points of the outer surface of bones,

and in a later contribution read before the same society, he stated:—

1. That where bones and teeth in the normal course of development exhibited such a surface in process of absorption, this consisted of a collection of closely aggregated superficial cavities or lacunæ.

2. That these lacunæ, the so-called lacunæ of Howship, were without exception filled by the many nucleated giant cells (*Myeloplacques* of Robin, *Riesenzellen* of Virchow). That, as a rule, in one lacuna there was one giant cell, exactly corresponding to it in shape and size, but that occasionally a single cell might fill more than one lacuna, or a large lacuna contain several cells; and,

3. He stated it as his opinion, that these giant cells did not arise from a transformation of the bone cells proper, but from an alteration of the formative cells of bone tissue (*osteoblasts*), and that they were the agents by means of which bone and tooth were normally absorbed. He therefore designated them "*ostoclasts*" or "*osteophagi*."

As an example of this normal or physiological absorption, I shall sketch the process as seen in the absorption of bone substance in the jaw, before a growing tooth sac.

A section of the jaw, prior to the formation of the sac, shows only a network of new bone tissue, the spaces being filled with embryonic connective tissue and bloodvessels, and the new bone itself, covered with a layer or layers of osteoblasts. These cells are round, or variously angular from pressure, have a granular appearance, possess one and sometimes two nuclei, and are generally nucleolated. As the tooth sac develops itself, cells having an altogether different appearance may be seen among the osteoblasts covering the new bone. They vary in size much, are larger than osteoblasts, have generally an irregular margin, throwing out processes which may branch and anastomose with one another, or with the processes of a neighbouring cell. They may also be fusiform or rounded, and devoid of processes. The cell substance is a coarsely granular protoplasm, more or less opaque. The nuclei, which are frequently nucleolated, vary in number from one or two to many. The granular opacity of the cell sometimes renders it difficult to distinguish any nucleus. Clear round or oval holes are often to be seen in the protoplasm. These vary in size. The larger and more distinctly defined of these, in the centre of which a debris resembling fatty particles is sometimes to be detected, appear to be originated by a disintegration of the nucleus; the smaller and less distinct, by a disintegration of minute portions of the protoplasm. May it not be, that by this means the giant cell disappears, when it ceases to exercise its destructive function? Corresponding in size and shape with these cells are concavities in the edge of new bone, on which they are seated, whose margin before they made their appearance was quite even. These are the lacunæ

of Howship, which, when unoccupied by a giant cell, may frequently be seen to be filled with a granular debris and free nuclei, possibly the remains of such a cell. By this process of lacuna formation or excavation, the bone substance is gradually cleared away before the advancing tooth sac. After the irruption of the tooth, these cells gradually disappear, and osteoblasts as at first clothe the bridges of bone, whose formation instead of absorption again proceeds. In explanation of these phenomena, Kölliker says, in the article alluded to, "One can conceive that from pressure of the growing tooth sac, an irritative condition is given rise to in the osteoblastic layer, in consequence of which its elements transform themselves into myeloplacques, at the same time, from yet unknown causes, assume a new function, and perhaps produce a ferment."

As the most telling argument in favour of his theory, Kölliker brought forward the fact of the occurrence of his osteoclasts on the ivory pegs used in Dieffenbach's operation for pseudoarthrosis. Billroth showed that such pegs driven into living bone, on removal presented an eaten-out, uneven surface, which he attributed to absorption from pressure of granulations. In a specimen of such a peg received from Linhard, Professor of Surgery in Würzburg, Kölliker found not only typical lacunæ of Howship, but these filled with large many-nucleated myeloplacques; and, in a specimen sent to him by Billroth, beautiful lacunæ, and in many places human bone substance, deposited on the eroded surface of the animal ivory. Arguing from these facts, and from the occurrence of giant cells in many places, e.g., on the walls of all the larger medullary spaces in developing bone, on the boundaries of the cranial sinuses, etc., where they seemed to play an active part in the absorption of bone and the production of spaces, Professor Kölliker assumed that they would be found to be the active agents in cases of pathological absorption, e.g., from the pressure of aneurisms or tumours.

Not long afterwards, Dr Wegener, late first Assistant in the Pathological Institute of Berlin, published a paper in the *Berliner Klinische Wochenschrift* for 20th May 1872, and later, another in *Virchow's Archives* for 10th January 1873, in which he described certain cases in which the inner surface of the internal plate of the cranium had a peculiar rough feel to the finger, and presented a much eroded appearance. In these cases, an appreciable layer of a soft material was deposited between the bone and the dura mater, which on microscopic examination was found to consist chiefly of giant cells. He also mentions cases of bone absorption from aneurismal pressure, in which he found giant cells interposed between the sac of the aneurism and the bone, to be the evident agents of this absorption. He believes, too, that they cause the attenuation of the walls of long bones in old people.

Professor F. A. Kehrer, in an interesting contribution "On the Indentations (*impressionen*) of the Skull in the Newly-born," published by the *Oberhessische Gesellschaft für Natur und Heilkunde*,

in speaking of the means by which driven-in portions of bone are spontaneously replaced, mentions :—

1. The elastic stretching or tension of the cranial bones, with the pericranium and dura mater, called forth by the inward pressure.

2. The counter pressure of the compressed brain; and,

3. In cases of longer standing, where reposition has not immediately taken place, the absorption of the convex inner surface of the bone pressed inwards by means of Kölliker's osteoclasts, and the gradual filling up of the concave outer surface by the formation of new bone, i.e., by means of osteoblasts. The last he proved by experiments on kittens, which showed that an almost complete restitution of a deep parietal indentation was possible in the course of a few months. In transverse sections of such a bone, the uppermost layer was seen to pass continuously into that lamina of the parietal bone perforated by numerous vessel canals, and to distinguish itself in nothing from the intact surroundings. Under this, however, there was a layer of bone, in shape somewhat fusiform, or a modified triangle with a very long overlying base, the bony framework of which passed over into the former lamina without sharp demarcation. Carmine coloured this layer more weakly, while the green of chromic acid, in preparations treated with turpentine and damar varnish, appeared here darker than in the rest of the bone, or in the upper layer. The medullary spaces and vessel canals in this under layer, as in the upper layer, were clothed with osteoblasts. On the dura mater surface of the bone were to be seen lacunæ of Howship, with osteoclasts. That giant cells are also the means by which sequestra are separated from living bone is exceedingly probable. On examining a fresh sequestrum from a case of necrosis of the tibia in a boy of seventeen years of age, a patient in Professor Spence's wards, I found Howship's lacunæ covering all aspects of the sequestrum, both the external rough surface accessible to the probe, and those towards the medullary cavity. There were perfect many-nucleated myeloplacques floating about in the general mass of blood-corpuscles and pus on the field of the microscope, but I cannot positively state that I saw any occupying the lacunæ. This, however, is not matter of surprise, for the sequestrum having been separated, the immediate function of these cells was at an end; and, as before mentioned, when this is the case myeloplacques disappear. Mr Stanley, in his "Treatise on Diseases of the Bones," thus speaks of the process of separation, and gives the views of John Hunter: "If, in a living animal, a bone is rubbed with caustic, or otherwise injured to the degree of causing necrosis of its surface, a red line in the contiguous living bone will speedily appear, just as increased vascularity shows itself in the skin immediately around an eschar. In either instance, an increased vascularity of the living bone contiguous to that which has perished, is the first stage in the process of separation. Along the red line in the living bone, a groove between it and the dead bone is then formed, which gradually deepens until the separation of the dead bone is complete."

Mr Hunter observed that this groove is formed by the absorption of the living bone adjacent to the dead bone, and that the earthy matter of the living bone first disappears; consequently that the bone is softened before the groove appears in it. Whilst the dead bone is separating by the gradual deepening of the groove between it and the living bone, the groove becomes filled by granulations growing from the exposed surface of the living bone. Hence in the section of a bone, where exfoliation is in progress, a soft vascular tissue is found to occupy the line of separation; and in detaching a piece of dead bone, the subjacent surface of the living bone is found to be covered with a soft, velvet-like layer of very vascular granulations; further, upon the exfoliated surface of dead bone small circular hollows are seen, which exactly fitted to the granulations from the living bone." The red line around the dead bone marks the point at which the "irritative condition," spoken of by Kölliker, is given rise to, and this in its turn is the stimulus which excites the formation of giant cells, which, doubtless, by their absorption of the bone in the area of such irritation, produce the groove which ultimately separates the dead from the living bone. The small circular hollows on the exfoliated surface mentioned by Hunter were evidently lacunæ, or groups of lacunæ; and from his having observed that granulations in the living bone exactly coincided with these, I presume he concluded that the granulations produced them; an explanation of bone absorption, as before mentioned, given by Billroth of Vienna, in the case of the eroded ivory pegs (v. *Die Allgemeine Chirurgische Pathologie und Therapie*).

The source of origin of giant cells is a contested point. Virchow (*Cellular Pathologie*) maintains that bone substance may simply transform itself into these cells (*Markzellen*). Rindfleisch (*Lehrbuch der Pathologischen Gewebelehre*) believes them to be altered bone cells, liberated by a softening of the bone tissue. Though allowing that Howship's lacunæ may be occupied by giant cells, he does not say that they are produced by them, but insists that both lacunæ and the canaliculization of bone, described by Volkmann in a case of osteitis, are owing to an increased transport of fluid nourishment to the bone, the corpuscles and their anastomoses being widened, and the intercellular material absorbed. This absorption and widening, according to him, follows naturally in the direction of the strongest stream (of fluid nourishment), and it may occur, that in one case the fluid, advancing with varying rapidity on the border of absorption, forms at a certain point a bay (*bucht*), while next to this a jagged point or promontory of bone remains, thus constituting a lacuna; in another case, the stream breaking through the bridge of bone between neighbouring medullary canals, constitutes the process of canaliculization. Wegener says, investigations have not as yet been sufficiently extensively carried on to prove whether they arise from bone corpuscles exclusively, or, in part at least, by a proliferation of the connective tissue in the walls

of bloodvessels. Kölliker, however, maintains that they belong to the same category of elements as osteoblasts, and that they proceed directly from such. He is led to this conclusion from a consideration of the following facts:—

1. There exist between both kinds of cells transition stages, in the form of larger cells, with one or more nuclei.

2. Most bone surfaces, which at the time of absorption show osteoclasts, were originally clothed with osteoblasts; and direct observation proves, the at first individual, then ever more numerous appearance of osteoclasts among osteoblasts; and,

3. Absorption surfaces may again become growth surfaces, and osteoblasts follow on osteoclasts; whence he concludes there is a transformation of the latter into the former by division, as was before exemplified by the jaw in teething.

With these various views before me, I have endeavoured to study this subject without being prejudiced in favour of one more than another.

I have seen true bone-corpuscles in developing bone proliferate, and also osteoclasts lying in close apposition on the one hand to the border of absorbing bone, and on the other to a twig of capillary bloodvessel.

I have seen, too, intermediate forms between osteoblasts and osteoclasts, the latter appearing in ever increasing numbers among the former, till these had quite disappeared; and from having seen an osteoblast of large size, with a distinctly dividing, hourglass-shaped nucleus, I can easily imagine that thus the nucleus may multiply, and the cell protoplasm increasing in quantity, a true giant cell be given rise to. But though I have seen bloodvessels in the above close relation to osteoclasts, and stages intermediate between the latter and osteoblasts, much more frequently have I observed the close relation of the embryonic connective tissue filling a bone space to these giant cells.

Osteoblasts, as is generally maintained, among others by Ludwig Stieda, in an excellent monograph on the development of bone, proceed from this undifferentiated (*indifferentem*) embryonic connective tissue. This consists of nuclei frequently nucleolated, and, as a rule, with a process from each extremity of the nucleus, which gives the whole a fusiform appearance. The nuclei may, however, be naked. How then does a young connective tissue corpuscle develop into an osteoblast? Evidently, by its processes becoming broadened out and granular, so as to surround the nucleus. The naked nuclei have probably the power of producing this granular cell substance around themselves.

Though osteoclasts assume very various forms, they frequently appear as fusiform cells, with a granular cell protoplasm and nuclei varying in number. I believe these fusiform cells arise from embryonic connective tissue, just as osteoblasts do, the transformatory impulse in this case being stronger, both as regards the size

to which the cell attains, and the multiplication of its nucleus. This is seen in a section of the lower jaw from an embryo calf, which I have. But I believe there is yet another mode of origin of myeloplacques.

Bone spaces may be observed with here and there a giant cell occupying its lacuna in the edge of absorbing bone, entirely destitute of osteoblasts, but containing the nuclei of embryonic connective tissue. These nuclei, generally scattered, are here and there closely aggregated, and show an internuclear opacity, which, however, has not the distinctly granular appearance of the opaque cell substance of a fully-developed giant cell; but this appearance is in variable degree even in fully formed cells. Is it not possible, then, that the above aggregation of nuclei may be the first stage in the formation of such a cell; that these then prepare a cell material each around itself, which, coalescing with that round its neighbours, produces the many-nucleated giant cell? In other words, this may be said to be the progressive development of an embryonic corpuscle, in the first instance to an osteoblast, and then to a giant cell, by the fusion of a number of osteoblasts. This is not, however, an enunciation of the theory of simple transformation, as the space was devoid of osteoblasts. Unless, indeed, the molecular theory of development be taken into account, and the histolysis of the osteoblasts freeing their nuclei be the first step in the histogenesis of an osteoclast, which again argues the inactivity, if not absence, of embryonic connective tissue—an absence, in developing bone at least, which I have never observed.

With regard to the transformation of an absorbing into a growth surface, I am inclined to think that on the removal of the stimulus to absorption, *e.g.*, in the above specified example on the irruption of a tooth, the osteoclasts disappear by molecular disintegration, and that the formative osteoblasts which succeed them arise anew from embryonic connective tissue corpuscles.

Lastly, as an argument in favour of the origin of giant cells, either from embryonic corpuscles, or by a proliferation of the connective tissue in the wall of a capillary bloodvessel, I would adduce the occurrence of giant cells in the knots of miliary tubercle. These, according to Wagner (*Handbuch der Allgemeinen Pathologie*—Uhle and Wagner), constantly occur, generally in the centre, rarely at the periphery of the knot, and are one or more in number. They vary in size and shape, are darkly granular, much branched, and contain very numerous (20, 50, 100) nuclei. They are also met with in granulation tissue, and in various tumours. The actual means by which giant cells absorb bone is as yet unknown.

ARTICLE IV.—*On Abscess of the Larynx simulating Croup.* By WILLIAM STEPHENSON, M.D., F.R.G.S.E., Physician to the Royal Hospital for Sick Children, Edinburgh.

(Read before the Medico-Chirurgical Society of Edinburgh, 21st May.)

THE larynx in children is subject to several acute diseases, which, although pathologically distinct, are yet so alike in the symptoms, that their diagnosis can at times be determined only by a post-mortem examination. There is no class of cases, moreover, where, in the matter of treatment, the power of accurate diagnosis is more desirable or can be of greater importance.

In the present paper it is not my intention to enter upon the consideration of those cases where the mucous membrane is the subject of morbid action. There is another class of cases which present the same laryngeal obstruction, which prove as fatal, and which, from their greater rarity, are extremely liable to be mistaken for the former, but which differ entirely from them, in the fact that the morbid action is extra-laryngeal. It is to this class I would refer.

Since the year 1819, when Dr Abercrombie recorded three cases, and more recently by the observations of Fleming of Dublin, Bokai in Germany, and Allen of New York, the affection known as retropharyngeal abscess has become fully recognised. It is described at length in all our recent works on Pediatrics, so it is unnecessary to enter on details here. In some cases the symptoms could hardly be mistaken for those of croup, but there are others where the resemblance is very close, and an error in diagnosis has been made. The more important diagnostic points are the following:—

1. *The affection is more gradual in its onset, and does not endanger life so rapidly as croup.* Dr Abercrombie, in recording his first case, says—"I considered the disease as croup, but, on inquiring into the history of it, I was told that the affection had existed in nearly the same degree of violence for eight days, and the child's health not otherwise impaired. Concluding, from these circumstances, that the disease could not be croup, I was led to a particular examination of the throat, etc."¹

2. *Difficulty in swallowing is, in most cases, a prominent symptom, and any attempt at deglutition at once induces a paroxysm of dyspnœa.* Occasionally, though rarely, dysphagia has been absent.

3. In abscess, *change of posture, and especially the horizontal position, aggravates the dyspnœa*; and, like the act of deglutition, is apt to induce a paroxysm of suffocation.

4. The cough is low and hoarse, and has not the clanging brassy sound of the early stage of croup. These characters, taken together with the history of the case, should be sufficient to place one on guard for the existence of extra-laryngeal disease.

¹ (Three) Cases of Severe Dyspnœa in Children, connected with a Singular Affection of the Throat. Edin. Med. and Surg. Journ., April 1819.

Whilst retropharyngeal abscess has obtained a distinct place in literature, abscess of the connective tissue in more immediate relation to the larynx is unnoticed by English writers. The French, also, make no reference to it, so far as I have ascertained. Rilliet, Barthéz, and Bouchut do not mention it. Vogel states that "abscesses of the larynx are said to have occurred as sequelæ of typhoid fever; though," he remarks, "I have not met with a single laryngeal abscess in my dissections."¹

Schiele² has recorded a case of this kind, and Möller³ describes a case of external and internal abscess of the larynx in a child, terminating favourably. Dr Thin of Penicuik, now of Shanghai, records a case of abscess of the larynx in a boy aged seven.⁴

In addition to these cases, Dr Joseph Bell informs me that he was once summoned to perform tracheotomy in a case supposed to be croup, in which he discovered an abscess pointing in the mesial line immediately below the larynx, which he opened, with the immediate relief to respiration and the final recovery of the patient. He had not, however, preserved any notes of the case.⁵

The following three cases have come under my own observation. The first occurred nine years ago, in the Children's Hospital, but was not under my care. I have, however, extracted from the case-book the following particulars :—

CASE I. Anne M., æt. 4, was admitted to the Royal Hospital for Sick Children, 26th January 1864. The patient had scarlatina three weeks before. She had not much sore throat at the time, but

¹ Vogel, *Dis. of Children*, p. 186.

² Observation d'Abscès au Larynx à la suite d'une Fièvre Typhoïde. *Annuaire par Noïrot*, 1859, p. 214.

³ Möller, *Ext. and Int. Abscess of a child terminating favourably*. *Königsberg Med. Yearb.*, vol. ii., 1860, p. 160.

⁴ This case differs from those that follow; the special particulars are worthy of note, in connexion with the subject. "Boy, aged 7, for a considerable period had been more or less affected with difficulty of breathing. . . . On two occasions was seized with spasmodic dyspnoea of a very alarming character, supposed to have been induced by deranged digestion. . . . Scrofulous habit, body emaciated and ill developed. He was in good spirits, and spent most of his time out of doors. His only symptom was constant dyspnoea, both inspiration and expiration being accompanied by a loud hissing noise, so loud as to disturb the sleep of those who slept in the same apartment. His voice was thick and indistinct, but *there was no hoarseness*. He had an excellent appetite, and *swallowed without difficulty*. He died in a paroxysm of dyspnoea, which occurred shortly after the passage of a probang with solution of nitrate of silver. Tracheotomy was performed, but the boy sank. *Autopsy*.—The rima glottidis was completely filled with thick glutinous purulent matter, and immediately beneath, and partly involving the right vocal cord, there was an excavation about the size of a pea, filled with pus of a similar nature."—*Ed. Med. Journal*, December 1860.

⁵ After the reading of this paper before the Society, Dr Handyside stated that he had, many years ago, been called upon to perform tracheotomy for supposed croup. After cutting down upon the trachea he discovered a large abscess lying somewhat posterior to it, which, being opened, at once gave relief to all the symptoms of croup. The child recovered.

about a week ago the glands of the neck began to swell, and she had some difficulty in swallowing. She was getting better, however, and the swelling passing off, when two days ago she was seized with difficult and noisy breathing, accompanied by dysphagia.

On admission, patient was evidently suffering from laryngeal obstruction to respiration. The breathing was laboured and stridulous—this being at times considerably increased. She had a hard cough, coming on in fits, but no clanging. The face is dusky, the lips blue, the extremities cold, the pulse quickish and very feeble. The child is struggling for breath, *and is unable to lie down*. It could not be ascertained that there was any pain about the larynx, or fulness on looking into the back of the throat. It is seen to be somewhat congested. There is a slight enlargement of the throat felt externally on right side. *Deglutition is difficult. It brings on cough, and increases the dyspnoea.* On auscultation of the chest nothing can be heard. No dulness on percussion.

During the next two days she had frequent suffocating attacks, some of which nearly proved fatal. After this the respiration was easier, but the child gradually sank, and died four days after admission.

Post-mortem Examination.—There was pneumonic consolidation of lower lobe of left lung. The right lung was congested. The bronchi were filled with muco-purulent secretion. The trachea was healthy. On examining the larynx, a small abscess was found at the outer side of the right thyroid cartilage, laying bare the upper margin, and extending to the superior cornua. It had opened internally, probably on the 28th of January, after which date the respiration was freer, and there was no return of the suffocative attacks. Death was evidently caused by the secondary lung complications, and the enfeebled state of the child.

This case, which I had the good fortune to see, was very instructive to me, being the first of the kind I had seen, and having before death regarded it as one of secondary croup. Looking back upon it, with the knowledge gained by the post-mortem examination, and comparing it with the symptoms of retropharyngeal abscess, one can readily see how its true nature might have been surmised, although it could not have been positively diagnosed. At the same time it opens up the important question, might tracheotomy, early performed, not have prevented the lung complication?

CASE II. Ann Maria L., 2 years old, was admitted to the Children's Hospital 22d June 1872, supposed to be suffering from croup. The child was weakly and delicate-looking, and still unweaned. The following history was given by the mother: Three weeks previous to admission the child had an attack of smallpox. When convalescent a swelling appeared at the one side of the neck, and two days after another swelling on the other side. Poultices were applied, which appeared to relieve the child, and the swellings

subsided. A week ago signs of difficulty of breathing began to show themselves, accompanied by a wheezy noise, the voice also being affected. The difficulty of breathing increasing, together with inability to take the breast, the mother applied to the Hospital, and the child was visited by the out-door physician, who recommended it for admission.

Being regarded as a case of croup, and the difficulty of breathing so urgent as to demand tracheotomy, I was at once summoned. I found the child suffering from great dyspnoea, inspiration being principally affected, and laboured, accompanied by a croupy sound and a hoarse cough, without clangour. What most struck me when I saw the child was the steady erect position with which she sat up in bed; which, with the history I obtained, at once made me suspect I had not to deal with true croup.

Upon examining the larynx I detected a small swelling immediately below the thyroid cartilage, which I afterwards ascertained had been observed two days before admission. It felt like a small cyst. Puncturing it with a small trochar, pus escaped. I then opened it more fully with a bistoury, and, as near as could be ascertained, three or four drachms of pus were withdrawn. Respiration at once became greatly relieved, the child lay down quietly in her mother's arms, and shortly afterwards took the breast. The breathing, however, though relieved, never became quite free, inspiration being slightly laboured, with a slight laryngeal sound. Stimulants and nutritives were administered, but the child gradually sank, apparently more from the weak cachectic condition than from the laryngeal symptoms.

Upon examining the larynx after death, the epiglottis was found slightly oedematous; the larynx was quite patent, and the mucous membrane healthy. There existed in front of the thyroid cartilage a sac containing some pus, and extending upwards, on both sides, to the upper margin of the *alæ* of cartilage, the pouch on the right side being somewhat larger than that on the left. The cartilages, though denuded, presented no appearance of necrosis; at one point only there was a slight erosion.

CASE III. A weakly delicate child, *æt.* 18 months, was brought to my house suffering from a glandular swelling under the lower jaw, left side. No cause for the swelling could be detected, and the mother was advised to bathe it with warm water, and rub it with warm olive-oil. A fortnight after I was sent for to see the child. I found her suffering from difficulty of breathing, which had been gradually coming on for two days. Respiration was accompanied by a loud stridulous sound, audible throughout the room, which, with the dyspnoea, was at once suggestive of croup. There was a hoarse cough without clangour. The child was restless, but lay quietly in bed during the visit. There was no dysphagia. The throat was examined, but seemed quite healthy.

With the last case, which had occurred only two days before, fresh in my mind, I made a careful examination of the neck. The glandular swelling had wholly disappeared, but there appeared a fulness, but no hardness, in the lower part of the neck, especially during expiration, and the trachea seemed to be pushed slightly out of the mesial line to the right. This fact, with the history of the case, led me to suspect that I had again to deal with an *extra-laryngeal* affection. There was no stiffness or pain in the neck. Fifteen drops of aromatic spirits of ammonia were ordered to be given frequently, and warm poultices to be applied to the neck.

The condition of the child remained pretty much the same for a week, and the idea of croup was consequently negatived. The displacement of the larynx became rather more marked, and expiration more forced; with each performance of the act there rose a soft bulging forward of the integument along the inner margin of the sterno-mastoid muscle, reaching as high as the thyroid gland, receding again with inspiration. It had more the appearance and feeling of air than fluid; no hard swelling or fluctuation could be felt.

At the end of the week I determined to explore with a needle, but failed in finding pus. The poulticing, however, was continued, nutritives and stimulants freely given, and the child's condition continued much the same. Auscultation of the chest revealed nothing, as the loud moist laryngeal ronchus masked all other sounds.

At the end of the second week of the attack, I at last could feel a small rounded soft swelling, about the level of the isthmus of the thyroid, and at the outer margin of the sterno-hyoid muscle. Pushing in a small trochar, pus escaped, and on making a free incision, fully four ounces of pus were discharged. Inserting the handle of the bistoury I had used, I found it passed readily downwards its whole length, nearly three inches.

Steady improvement in respiration resulted, the abscess discharged freely for four or five days, and slightly for a fortnight. The child continued very feeble for many weeks, but subsequently regained her ordinary state of health. Whenever, however, she cried, inspiration was accompanied by a loud crow; her voice also has continued somewhat feeble.

Under the title of submucous laryngitis, MM. Rilliet and Barthez detail a case¹ which I am inclined to regard as of the same nature as the two first cases I have recorded. They state it is the only one of the kind which they have met.

The boy, $4\frac{1}{2}$ years old, was in a cachectic state following measles. There was swelling of the submaxillary glands on both sides of the neck; that on the right suppurated. Pain on deglutition set in, without visible signs of inflammation of the fauces. Respiration was laboured, and accompanied by a croupy sound. There was complete aphonia. At the autopsy the larynx was in the fol-

¹ *Maladies des Enfants*. Paris, 1861, vol. i. p. 385.

lowing condition: The epiglottis was thickened, the aryteno-epiglottid fold was greatly swollen, the orifice of the larynx was considerably narrowed. Upon incising the tumefied parts, there oozed out, on pressure, a yellowish purulent fluid. The mucous membrane itself presented no signs of inflammation. The submaxillary region was infiltrated with a great quantity of purulent fluid. In this case death had occurred before the full formation of the abscess; there was some purulent matter.

In the two first cases I have recorded, and in that of Rilliet and Barthez, the affection was secondary; and in all of the cases the laryngeal affection followed enlargement of the submaxillary glands. There is no reason, however, for supposing that it was also glandular in nature.

Comparing them with the symptoms which peculiarly mark the affection of retropharyngeal abscess, we see a marked similarity. The more gradual onset of the laryngeal symptoms, as compared with croup; the same pain and difficulty of deglutition; the paroxysms of dyspnoea which ensue on that act; and, lastly, the preference for, and comparative relief obtained in, the erect position. Any one of these characters, then, becomes of the greatest importance when met with in cases of acute laryngeal obstruction. Though such cases are rare, the fact that they do occur must ever necessitate a careful examination in every case; and it is fortunate indeed that they are marked by symptoms sufficiently distinct as to enable us to infer their existence. The more, that in such cases accuracy of diagnosis is half the treatment.

Postscript.—While this paper was in the press, the Editor of this Journal has forwarded to me the *Philadelphia Medical Times* of 14th June 1873, containing a paper on "Abscess of the Larynx in Young Children," by John S. Parry, M.D., Accoucheur to the Philadelphia Hospital. As the periodical may not be accessible to many in this country, and in order to make this paper as complete as possible, it may be well to subjoin a short résumé of the article.

Two cases are recorded. The first, aged 4½ months, was well nourished, and, before the occurrence of the affection, had been only cross and irritable for two or three days. The second, aged nine weeks, had suffered from a week old from erysipelas, spreading over the whole body, followed by an abscess of the scalp. For a month he had been improving; for ten days he had had wheezing in his throat. In both, the head was thrown back and the neck rigid; inspiration was long, difficult, and stridulous; expiration easy and noiseless; little or no cough, the voice hoarse and whispering. They could not take the breast; the first was unable to swallow, the second could only with great difficulty. The larynx in both was thrust forward so as to form a decided prominence in the neck. After two days, in the first, slight fluctuation was detected, and an incision made in the mesial line over the thyroid cartilage; two

drachms of pus escaped, with immediate relief to all the symptoms; the child made a good recovery. The second child continued to grow worse during the succeeding night and died the next day; no operation was performed. The autopsy revealed an abscess on both sides of the thyroid cartilage, meeting in front, exactly according to that in Case II. of this paper. "The perichondrium was separated from the thyroid cartilage upon both its inner and outer surfaces. Both surfaces of the cartilage were eroded and rough, while its tissue was softer than it is when healthy. The cricoid cartilage was perfectly healthy. The other organs of the body were healthy." Dr Parry calls attention to the absence of any reference to this affection in our text-books, and remarks on the similarity of the symptoms to those of retropharyngeal abscess, and the diagnostic points between it and cases of intra-laryngeal affection, which accord with those given in this paper.

ARTICLE V.—*Address to the Scottish Midland and Western Medical Association. Read at the Annual Meeting on 30th July 1872.* By JOHN BOYD, M.D., Slamannan, retiring President.

IN looking at the present aspect of the medical profession in Scotland, we may remark that a considerable number of those qualified by abilities and attainments, by matter and manner, to succeed in town practice, prefer a country life, with its toils and exigencies, and invigorating exposure to all weathers. To these the existence of those bits of town stuck into the country, in the shape of miners' rows and ironworkers' squares, furnish a field for professional exertion and a source of emolument such as never could be derived from a sparse and frugal agricultural population. From the multiform accidents and protean forms of disease that are incident to mining practice, we enjoy opportunities of acquiring medical experience, and especially operative surgical dexterity, little inferior to those afforded by hospital connexion. We have excellent chances of bringing to the test of practical experiment the pathological and therapeutical views and ideas propounded by the magnates of the healing art who occupy, or are preparing to occupy, professorial chairs. But it must be admitted that our position as rural practitioners is not the most favourable to original scientific research, inasmuch as we may at any moment be called upon to lay aside the microscope, the test tube, or the writing desk, being summoned with equal vehemence to arrange for operation in a compound fracture of the femur, to allay the pangs of dentition in a six-months' child, or to still the intestinal commotion in some jolly goodwife whose previous vesper meal has been rather too generous.

One of the leading advantages to the practitioner in mining dis-

tricts is, that from the identity of position of the population with whom he has to deal, their social habits, liability to injury, and the rapid diffusion of contagious diseases among them, requiring ready access to medical skill, a system has grown up of remunerating these medical attendants by small fixed fees deducted from their fortnightly pays; a sum so moderate that they never feel it, but being certain, could be depended upon by the recipient, who was thereby saved from the risks and harassing worry of collecting accounts from his clients. This system, from the very necessity of the case, has sprung up from a very early period of the mining industry, the development of which has conduced so materially to the advancement of our national wealth and power; and has been found so beneficial to all parties that, however changeable and malcontent miners might be occasionally as to the individual medical attendant on the work, they remained firmly attached to the system, in despite of the efforts of some questionable parties to revolutionize or destroy arrangements which, on the whole, were found to be salutary and altogether essential to their comfort and safety. Indeed, the payment of 2d. a week per man, securing, as it did, the ready services of an efficient doctor, and a supply of everything in the way of medicines and medical appliances that the case required, not only for the workman himself, but his wife and all of his family depending on him, made it at all times an uncommonly good bargain for the miner; while, from the number of young unmarried men attached to each work, it was not unremunerative for the son of Galen himself. But the medical profits of coal and iron production came, in the course of time, to be seriously deranged by the results of mining of quite a different nature, in another hemisphere. By the discovery of gold in California, and subsequently in Australia, added to the more or less continuous outpouring of silver from Mexico, Peru, and Bolivia, there has been made such an addition to the supply of the world's currency, that money has lost very much of its purchasing power from its very abundance, leading to a diminution of from 25 to 50 per cent. in its relative value. The wages of labour and the price of commodities augmented in the ratio now referred to, and those, necessarily, whose income was fixed down to a certain figure, became in these latter days so much the poorer. By none was this more severely felt than by the doctors of coal and iron works. Not to speak of the augmented cost of every article of living, the rise of food, clothing, rent, and wages, our business expenses were enormously increased. As the population whose hygienic necessities we subserve are scattered over wide districts of country, we require horses and conveyances to reach them; and a good useful animal, which twenty years ago could be got for as many pounds, and kept twelve months for half as much more, requires now at least double the sum to purchase and maintain. To keep up with the progress of science we are obliged to make incessant additions to our libraries, besides delicate and costly surgical

instruments. But our drug bill was daily becoming a more serious item. Fiscal changes added decidedly to the charges of the numerous articles of *materia medica*, in the composition of which rectified spirits entered; to ethers the progress of chemical art caused an unwonted dearth, as for instance that indispensable alterative deobstruent, the iodide of potassium, which rose from a dozen of shillings to 38s. per pound. Now, there could be no such thing as economizing in this department. No medical man, when waiting on a serious case, could ever stultify himself so far as to withhold any medicine that the exigencies of the malady might require, no matter what the cost of it might be. This point merits attention more especially from it being a widespread prejudice among the class alluded to, that there are medicines of such transcendent potency and preciousness that we never dispense them for patients in the humbler walks of life; that when a death occurs, either the doctor, in their own phrase, "did not take up the trouble," or that he was so stingy that he withheld the medicine that would have saved the patient, because it was too expensive! This droll notion is not confined to home experience. A friend of mine, who practised for some years among the copper-miners of South America, tells me that it was a very common expression thereaway—"Tal medicina para los ricos, tal droga para los pobres"—such a medicine for the rich, such a drug for the poor. In fact, to apply one of the pithy proverbs that enrich the language now quoted, "*¡Quieran mejor pan que no de trigo!*"—they want better bread than is made of wheat.

Under such circumstances, a new arrangement for the remuneration of our services became so urgently needful that a combined movement of the whole of the medical profession connected with public works, it was felt, could no longer be delayed. Our isolated lives, and the inconveniences from want of intercommunication, were so forcibly experienced that the formation of the Scottish Midland and Western Medical Association did not occur a day too soon. What individual effort never could have effected has been to a great extent achieved, and a rise of 1d. per week per man has been the result. This very moderate augmentation has for the most part been cheerfully conceded by the workmen; and in general the coal and iron masters have testified their sense of the importance and value of our functions by promoting the attainment of our object as far as lay in their power.

The value of combined action was also felt when, in the proposed Master and Servants Bill before Parliament last session, it was intended to do away with the system of office deductions for doctors, as well as other matters. Organized influence made itself felt upon the Legislature, and the clause that threatened to produce an innovation, as disastrous to the workmen themselves as to the medical profession, was definitively withdrawn.

A large number of our members hold the position of parochial medical officers in their respective districts. As such, it is quite

within the range of eventualities that, under the influence of nepotism, of local prejudice, or that sectarian spite from which some rustic districts are not entirely free, attempts may be made to victimize any one of us by ejection from office without a shadow of reasonable cause assigned; exactly as occurred a number of years ago in the case of Dr Henderson of Fordun, who was dismissed because he made a post-mortem inspection of a pauper, carefully and decorously done, to enable him to certify understandingly the cause of death. The Colleges of Physicians and Surgeons took immediate action to overturn this disgraceful piece of parochial jobbery and injustice, and were backed by the medical public and the press. Ultimately the Board of Supervision was induced to order the rescinding of such proceedings, and reinstate that gentleman in office.

From our unavoidable attainments, the *savoir-faire* insensibly arising from our intimate relations with all ranks and classes, each one of us is usually in possession of a certain amount of influence on public opinion that would tell politically far beyond the weight of our personal votes, whenever it might seem advisable to exercise it. In the instance of a late general election, I am persuaded that, apart from general politics, the former member for Stirlingshire was unseated in a great measure from the natural resentment he excited among the medical profession from the active part he took in moulding a portion of the Vaccination Act—tying down the remuneration for vaccination at 1s. 6d. within a distance of two miles. As fixed at present, we may have to travel two miles to perform a minor operation requiring some care and skill, return home and pay another two miles' visit and return, before we can certify as to the success of the achievement; travelling in all eight miles for eighteenpence!—thus affording a glaring illustration of the tendency of a portion of the public to seek to compound for its own parsimony by demanding heroic generosity and self-sacrifice from the doctors.

Our friends of the legal profession amply vindicate their claims to the possession of some of the best brains in Scotland among them, by taking laudable care that, whenever legal duties come to be performed, sufficient provisions are sedulously laid down for exact and adequate payment of professional labours. But where the framing of Acts of Parliament are concerned, involving the interests of another profession, they cannot assume the merit of equal considerateness. When the final lot of humanity overtakes any member of the community, the physician in attendance is bound, under a heavy penalty, and without fee or reward, to ascertain by personal inspection that the defunct is really dead, and to fill up a schedule stating the name of the deceased, when last seen in life, when they died, how long they were ill, and what maladies primarily, secondarily, or tertiarily operated in bringing about the fatal event. Now, medical practitioners pay their full quota of taxation to the general expenses of the State, derive no State pay or privilege *quâ* doctors; and their being compelled, under a severe

penalty, to furnish important written information to the community, which the nation is as entitled as it is able to pay for, is most undoubtedly, so far as it goes, a contemptible and reprehensible breach of national honesty and fair dealing.

To remedy the grievances of which the above is a sample, and successfully assert our legitimate claims on the public and on Parliament, I have frequently felt that we would do well to copy the pattern that has been held out to us by our clerical brethren, in the stated and regular intercommunication which has done so much to organize and consolidate the Presbyterian mode of church government during the last 300 years. Our county branches might suitably and efficiently represent their Presbytery meetings; the Scottish Midland and Western Association would take its place as an important and influential Synod, subordinate to a General Medical Assembly, to be held fixedly or alternately in one of the university cities of the northern kingdom. This I suggest for the consideration of all Scotch doctors, being satisfied that the time is now ripe for a general movement of the whole profession in the direction thus indicated.

Permit me to conclude these observations, on the means which we are properly called upon to adopt for the due advancement of our temporal interests, by referring to the favourable reflex influence which the exercise of our vocation, in the great majority of cases, is calculated to operate, and does operate, upon our moral and spiritual natures. It is not to be denied that here and there and now and then are to be found individuals among us who, led away by passion and temptation, by intemperance and allied vices, fall lamentably short of that standard of rectitude and decorum which the public very properly demands of its medical advisers. "*Non ragionem di lor, ma guarda e passa,*"—Let us say nothing about them, but look and pass on. With these few and insignificant exceptions, I may justly claim for the average medical practitioner a gradual and progressive elevation of mental tone and tendency, brought about by the habit of caring much for others and comparatively little for himself—the custom of subordinating his comfort and convenience to the wants and benefit of those with whom his duty brings him into contact. The professional difficulties with which we have to cope single-handed bring out our nerve, knowledge, and self-reliance in no ordinary degree. A winter's country practice, during the prevalence of an epidemic, demands as much courage and self-devotion as are required by any military officer in an active campaign. All sham and subterfuge excite our contempt as soon as discerned, inasmuch as it is only in so far as we can catch hold of the truth and follow it up and out, that we are able to do good to our patients—the first and only way of being ultimately able to do good to ourselves. Our position, realizing in a great measure the prayer of the prophet Agur, removes us, on the one hand, from the temptations to sordid trickery involved by penury, and on the other

DIAGRAMS SHOWING THE EFFECTS OF THEINE ON THE RESPIRATION,
PULSATION OF THE HEART, AND TEMPERATURE.

DIAGRAM I.

*Showing the Effects of Theine upon the Respiration,
as ascertained in a Rabbit.*

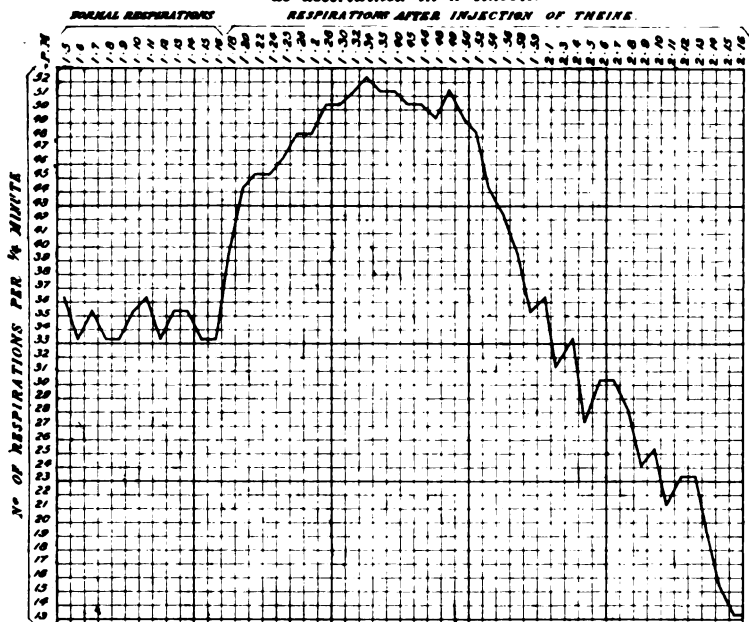


DIAGRAM II.

*Showing the Effects of Theine upon the Pulsations of the Heart,
as ascertained in a Rabbit.*

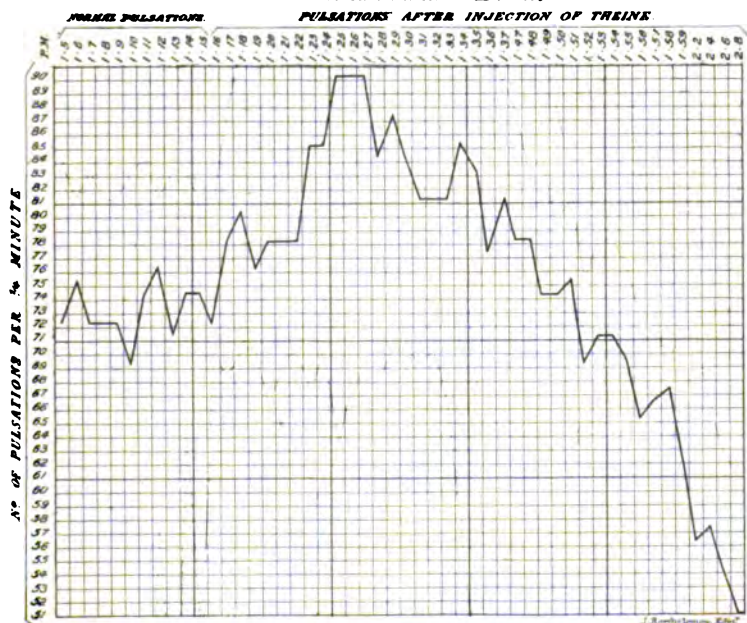
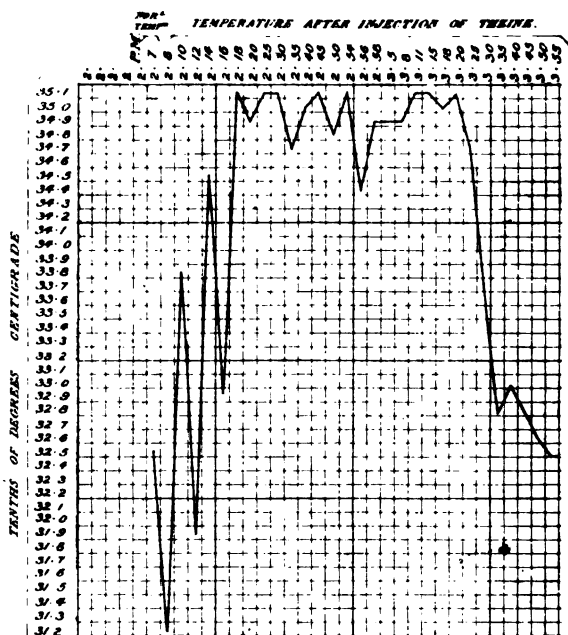


DIAGRAM III.

*Showing the Effects of Theine upon the Temperature,
as ascertained in the Ear of a Rabbit.*



from the tendency to ostentatious displays and luxurious self-indulgence, which we so frequently see accompanying the suddenly acquired wealth of these latter times. Our nervous and digestive systems are rarely implicated by the perils of hospitable profusion, as when we have the leisure *pour ces affaires-là*, we have not the means; when we have the means, we have not the time. While fully appreciating the refined courtesy and deference we usually receive from the high-class families we attend, we are but little exposed to the snobbish abjection to riches or social position *per se*. The millionaire millowner and the char-filler, the countess and the kailwife, the solitary scion of a wealthy historic line, and the fourteenth infantile production of Mrs Terence O'Shaughnessy, are, when they come under our jurisdiction, merely men, women, and children, subject to the same maladies and amenable to the same course of treatment. But, above all, we have the constant consciousness impressed upon us that our most elaborate investigations, and our best devised remedies, are efficacious only in so far as they are blessed by the Divine approbation—that Divine Providence alone can permit the fruition of our most earnest efforts in the service of science and humanity, and that we are enabled, as far as human skill and appliances can carry us, to follow in humility and singleness of heart the high and holy example given us by the Divine Physician, the Heavenly Healer, who, while upon earth, went about continually doing good.

ARTICLE VI.—*An Experimental Inquiry into the Physiological Actions of Theine, Caffeine, Guanine, Cocaine, and Theobromine.*
By ALEXANDER BENNETT, M.D.

THE following inquiry formed part of a graduation thesis given in by me to the University of Edinburgh on 5th July 1873, entitled, "Erythroxyton Coca, with an Experimental Inquiry into the Physiological Actions of Cocaine, Theine, Caffeine, Guanine, and Theobromine." The experimental research was conducted in the Physiological Laboratory of that University, and I have gratefully to acknowledge much kind assistance from Dr M'Kendrick.

Upwards of one hundred experiments were conducted on different living animals, chiefly frogs, mice, rabbits, and cats; and from these I have obtained results which seem to me of considerable importance. In the present paper, as brevity is insisted on, I propose, after a few introductory remarks,—1st, To give my general conclusions concerning the physiological actions of these neutral principles; 2dly, To describe in detail a few characteristic experiments; and, 3dly, To append a diagram showing the effects on the circulation, temperature, and respiration; also a series of tables showing at a glance the result of seventy-two experiments.

Theine, caffeine, and guanine, the neutral principles derived

from tea, coffee, and guarana, are so well known in the laboratory and in commerce that I do not consider it necessary to enter into a description of their preparation or chemical properties. It will suffice to state, that they are now proved to have the same ultimate composition, and, although procured from totally different sources, are chemically alike in every respect. The notation adopted by the most eminent chemists is for all $C_{16} H_{10} N_4 O_4$.

Cocaine, discovered by Neimann in 1859, is the proximate principle derived from the leaves of *Erythroxylon coca*, the narcotic of the Andes. This plant is employed in the form of a beverage by the natives of Bolivia and Peru as a stimulant and paratriptic. In this country the coca leaves are rare, and even in Paris where they can be obtained they are very expensive. Having, however, procured a supply from the last-named city, I placed it in the hands of Messrs Macfarlan and Co., of this town, who, after overcoming considerable difficulties, have with great care succeeded in obtaining a few grains of cocaine. When we compare this cocaine with theine, caffeine, and guaranine, we find that, if it is not identical, it is intimately related to them in chemical composition, its notation being represented by $C_{16} H_{10} N_4 O_4$ (Neimann).

Theobromine is the neutral principle of *Theobroma cacao*, from which the well-known beverage chocolate is obtained. The result of its analysis is $C_7 H_8 N_4 O_2$.

These five substances, although derived from totally different sources, closely resemble one another in chemical composition; it is therefore a question of the greatest interest to ascertain whether or not they also produce similar physiological effects. This would be all the more important to establish, as all these bases are the proximate principles derived from the beverages of different nations. It has hitherto been supposed and stated by many authors, that theine and caffeine are inert substances, and that the physiological effects of tea and coffee are not due to these neutral principles, at all events in a state of isolation. For example, Schleiden says in his lectures, that "the experiments hitherto instituted by physicians and chemists have as yet furnished no evidence of special actions resulting from the administration of large quantities of pure theine; the substance therefore appears devoid of any striking action on the animal economy."

In recent times, the therapeutical action of some of these substances has attracted the attention of the profession, and guaranine more especially has been employed by physicians as a stimulant and tonic, and in the treatment of sick headache, with, it is said, good results. But I am not aware that until now any series of observations have been instituted to demonstrate the exact physiological actions of these drugs, either upon man or the lower animals.¹

¹ I am aware that recently Drs Cogswell, Prichard Davies, and others, have administered theine to animals. Their experiments, however, are too few in number, and their conclusions, therefore, too imperfect, to warrant us accepting as anything like a complete investigation of the subject.

From the experiments conducted by myself I have arrived at the following conclusions:—

1. The physiological actions of tea, coffee, guarana, coca, and cocoa are mainly, if not entirely, due to their proximate principles.

2. Theine, caffeine, guaranine, cocaine, and theobromine are powerful poisons, inducing a series of symptoms affecting the nervous, respiratory, circulatory, vaso-motor, and glandular systems, which terminate, if the dose be large enough, in death.

3. These five principles are to all appearances identical in physiological action.

Note.—It is to be noted, that owing to the extreme scarcity of cocaine, and the very small quantity obtainable, the observations with this substance were confined almost entirely to frogs and mice, and only in one instance was it administered to a rabbit; accordingly the experiments were not so extensive as in the case of theine, caffeine, and guaranine. For this reason also, the phenomena connected with the temperature and the glandular secretions were not so completely demonstrated. In every other respect, cocaine had similar actions. The same may be said of theobromine, of which I was unable to obtain a large quantity.

4. In small doses, not ending fatally, these five substances produce, 1st, cerebral excitement, not succeeded by coma; and, 2d, partial loss of sensibility.

5. In large doses they produce, 1st, cerebral excitement; 2d, complete paralysis of sensibility; 3d, tetanic spasms and convulsions; and, 4th, death.

6. They paralyze the entire posterior columns of the spinal cord, also the entire system of peripheral sensory nerves; but the anterior columns of the cord and the peripheral motor nerves are not paralyzed.

7. They frequently produce convulsions of a clonic character, but occasionally they cause tetanic spasms, which latter are sometimes so severe as to cause opisthotonos. There is at first sight a resemblance between these spasms and those following the administration of strychnia. But in the case of strychnia, the action of the poison is limited to the spinal cord, the reflex function of which is so much excited that the slightest touch causes powerful spasms. A poisonous dose of theine, caffeine, etc., on the other hand, paralyzes the sensory nerves so that external irritations do not affect the cord; but, notwithstanding, there are strong spontaneous spasms which are probably caused by the action of the drug on the cord itself, and which spasms are not to be considered as reflex in their nature.

8. They do not produce muscular paralysis.

9. They at first increase, then impede, and lastly stop the respirations.

10. They at first increase, and finally diminish, both the force and frequency of the heart's contractions.

11. They produce at first contraction, and afterwards dilatation, of the capillaries and small bloodvessels, with stasis of the blood,

indicating first irritation and subsequently paralysis of the vasomotor nerves.

12. They affect the temperature by, 1st, slightly lowering; and, 2d, increasing it.

13. They usually produce contraction of the pupil.

14. They produce an increase of the salivary secretion.

15. They induce a peculiar form of tenesmus, accompanied by a copious discharge of clear mucus from the bowels.

The above are the conclusions I have arrived at, from a large series of carefully-conducted observations. In this paper it would be impossible for me to describe in detail an account of all of these, but appended will be found in tabular form seventy-two experiments, showing a characteristic specimen of the effects produced by each of the different doses, with the results of post-mortem examinations, etc. I shall content myself at present by giving in detail, as an illustration, a sample of some of the leading experiments, with the reasoning thereupon. As it has been already stated that theine, caffeine, guaranine, cocaine, and theobromine are to all appearances similar in physiological action, in the following cases I shall take theine as an example of the whole.

EXPERIMENT I.— $\frac{1}{8}$ gr. Theine. Frog. Recovery.

The $\frac{1}{8}$ gr. of theine, dissolved in 20 minims of water, was injected under the skin over the back of a healthy middle-sized frog. Almost immediately afterwards the respirations, which normally had been 80, were increased to 120 per minute. Seven minutes afterwards the respirations had diminished to 80 per minute. The frog was now distinctly sluggish in its movements. It made attempts to leap, but did so feebly. When placed on its back it recovered its normal position with difficulty. When its toe was pinched with a pair of forceps it drew up its leg. If its limbs were pulled out they were drawn up slowly. Two minutes after, these symptoms were increased, and in three minutes more the limbs were very weak, and the animal lay on its belly without their support. When placed on its back the frog was unable to recover its position, but lay there with its limbs drawn up; and when the skin or toes were pinched the limbs were moved, but sluggishly. The respirations had diminished to 40 per minute. Five minutes later the frog still lay motionless on its back with its limbs extended. All four legs were completely paralyzed, and they remained in whatever position they were placed. When any portion of the skin was pinched the animal made no movements. On touching the eyeball the eyelids did not close. The respirations had diminished to 26 per minute, and they were very irregular, some being deep and others superficial. There was marked congestion of the under cutaneous surface, which was of a reddish-purple colour. The mucous membrane of the mouth and tongue was also deeply congested. When the web of the foot was examined under the microscope (25 diam.), the

capillaries and small bloodvessels were found enlarged and engorged with blood, and there was in them complete stasis of the circulation. The heart was beating, but respiration had stopped. The frog remained in this prostrate condition for eleven minutes, when slight spasmodic movements were observed in the limbs. Four minutes later, it made feeble attempts to move its legs, and when its toe was pinched, it drew them up. Four minutes afterwards, the animal gave a very feeble leap, and tried to crawl along the table. It sat on all-fours, and when any portion of its skin was pinched it made attempts to move. The respirations, although still irregular, had risen to 20 per minute. Eighteen minutes afterwards, the frog jumped readily, especially if it was irritated. It croaked vigorously when touched, and in half an hour it was apparently in its natural state, with the exception of looking feeble. Next morning it was found in its normal condition.

Commentary on Experiment I.—In this experiment we find that shortly after the administration of the theine, the frog gradually lost motor power and all evidences of reflex action, the animal being reduced to a state of complete prostration. The number of the respirations was first increased and subsequently diminished, congestion of the cutaneous and mucous surfaces was produced, with enlargement of the small vessels, and engorgement of blood in the capillaries, with stasis of blood in their interior. From all these effects, the frog recovered gradually, after remaining under the influence of the drug for more than an hour.

This dose of theine may be considered as the largest that may be given to a frog without proving fatal, and the above description may be taken as the characteristic phenomena occurring in that animal after the administration of the poison. If smaller doses be given, similar symptoms ensue, but they are less violent and less rapid, in proportion to the amount.

It is now to be determined what portion of the nervous system is affected by theine, to account for the loss of motor power and reflex action:—

EXPERIMENT II.— $\frac{1}{16}$ gr. Theine. Frog. Death. Post-mortem.

The $\frac{1}{16}$ gr. of theine, dissolved in 30 minims of water, was injected under the skin over the back of a healthy middle-sized frog. A series of symptoms ensued, almost exactly similar to those in the preceding experiment, only somewhat more rapidly, and ultimately causing the death of the animal. On post-mortem examination, the limbs and body generally were flaccid. The skin, especially on its under surface, was congested, of a reddish-purple colour. On pinching with a pair of forceps any portion of the integument, no contractions of the muscles followed; but on the application of the electrodes of a Faradic current, there were strong muscular contractions. A flap of the skin was reflected about one inch square from the back, and when the electrodes were applied to the extremity of it, there were no movements; but when applied to the skin near the

muscles, these contracted feebly. When one of the limbs was removed from the rest of the body, and a reflected portion of the skin was treated in a similar manner, the same phenomena ensued.

The head was amputated, and on irritating the brain with the point of a needle, the muscles of the face and eye contracted. The exposed part of the spinal cord was also irritated with the electrodes of a weak Faradic current, and strong muscular contractions of the body and limbs ensued. A portion of the spinal cord, about a quarter of an inch in length, was then isolated by carefully removing with a pair of scissors the bodies of the vertebræ. On applying the electrodes of the weak current to the anterior columns of the cord, there were powerful contractions of the lower limbs; on irritating the posterior columns in a similar manner, these contractions were very feeble. On dissecting out the sciatic nerves, and pinching them with a pair of forceps, or applying the electrodes to the lower portions of them, strong contractions of the posterior limbs ensued; when to the upper portion, there were powerful contractions of the upper part of the body. On applying the electrodes to the gastrocnemius and other muscles, they contracted strongly. On opening the cavities of the thorax and abdomen, the heart was found beating feebly, at the rate of 64 per minute. All the internal viscera, especially the liver and bowels, with the mesentery, were deeply congested, and their vessels engorged with blood.

Commentary on Experiment II.—We find in this experiment not only enlargement of the small bloodvessels and capillaries, with engorgement and stasis of blood during life, as proved by inspection of the web of the foot under the microscope, but that, after death from a fatal dose of theine, there was a similar condition of the skin and internal viscera, showing that the drug paralyzes the capillary system, probably by its action on the vaso-motor system. This effect will subsequently be proved by experiments on mammals. It is also shown that the motor filaments of the brain and upper part of the spinal cord, the anterior columns of the cord, the motor nerves, and the muscles, were apparently in a normal condition; but that the posterior columns of the cord were partially paralyzed. To ascertain if the peripheral sensory nerves of the skin were also affected, a large flap of the integument was reflected. On applying the electrodes of a Faradic current to its extremity, no muscular contractions ensued, proving that the sensory nerves of that portion had been paralyzed. If the electrodes were placed on the reflected skin, close to the muscles, they contracted feebly; probably, however, owing to direct action of the electricity on the motor nerves of the muscles, which was proved not to be reflex by the same phenomena occurring when a limb was removed from the body. Whether the sensory nerves of the muscles also are affected or not, will be shown in a succeeding experiment.

EXPERIMENT III.— $\frac{1}{8}$ gr. Theine. Mouse. Death. Post-mortem.

The $\frac{1}{8}$ gr. of theine, dissolved in 10 minims of water, was in-

jected under the skin over the back of a white mouse weighing 3 drs. For fifteen minutes no effects were observed. When its tail was pinched, it uttered a cry, which it did before the drug was administered. At the end of this time it became sluggish in its movements, being weakened in all four limbs, but especially in the posterior ones. It could only crawl along the table, but could not run. Five minutes later, when placed on its back, the animal lay there with limbs occasionally kicking; and it turned over on its belly with great difficulty. On pinching its tail it did not cry, but struggled and attempted to get out of the way. Five minutes after, the mouse lay helpless on its side. The under surface of the body, the feet, legs, and mouth, were much congested and very red, the ears not markedly so. When the foot was pinched, the animal pulled away its leg. Five minutes later, when placed on its back, it lay there with limbs occasionally moving, and scratching itself with its toes. When its tail or skin was pinched, the animal made very slight efforts to move. When its toe was pinched, it drew its leg out of the way. It did not seem to feel a pinch of the ear. When the eyeball was touched, the eyelid did not close. When its face was touched, the muscles did not move. Congestion of the skin was still considerable. Twenty minutes afterwards, the animal had gradually become prostrate. When any portion of the body was pinched except the toe, there were no signs given of sensation. When the toe was pinched, there were slight muscular contractions of the limb. Congestion of the cutaneous surface had nearly disappeared, and the animal lay helpless and immovable. Respiration had stopped, and ten minutes later the mouse was dead.

Immediately after death, an examination of the body was made. The heart was still beating feebly; the integument, the thoracic and abdominal viscera, were all deeply congested, and their bloodvessels engorged with blood. On amputating the head and irritating the upper portion of the spinal cord with the point of a needle, there were feeble contractions of the muscles of the body generally, which were much increased on applying to the same place the electrodes of a weak Faradic current. On irritating the brain in the same way, the muscles of the face contracted powerfully. About a quarter of an inch of the cord was carefully dissected out by removing with a pair of scissors the bodies of the vertebræ. On applying the electrodes very gently to the posterior columns, there were slight contractions of the muscles of the lower limbs. On touching the anterior columns, there were strong contractions. On exposing the sciatic nerves, and pinching, or applying the electrodes to them, the limbs were powerfully contracted. On irritating the muscles, they contracted strongly under electrical stimulus. The membranes of the brain were found much congested, but its substance appeared healthy. The spinal cord was not congested, and seemed in its normal condition.

Commentary on Experiment III.—In this experiment, it is seen

that the effects of theine on a mammal are similar to those produced on a frog. The mouse lost its motor power. When irritated or pinched, it seemed to have lost its sensibility. Respiration was stopped, and complete prostration and death ensued. There was great congestion of the skin and internal organs, pointing to the influence the drug has upon the vaso-motor system. There was congestion of the membranes of the brain, which explains the cerebral excitement which is occasionally, although not in this instance, met with after the administration of theine. As in the preceding experiment, the motor filaments of the brain, cord, nerves, and muscles were intact, while the sensory filaments of at least the spinal cord were paralyzed. The slight movements produced by irritating the posterior columns were probably due to transmitted electricity, owing to the small size of the cord of a mouse. This was afterwards proved to be the case by experiments on larger animals. All these facts prove that the loss of motor power of the animal during life was not due to paralysis, either of the motor columns of the spinal cord, the motor nerves, or the muscles. They show, however, that there is at least paralysis of the sensory columns of the cord, and whether the peripheral sensory nerves are affected or not, will be shown in a following experiment. Death probably resulted from paralysis of the nerves connected with the respiratory processes. In this experiment, the mouse died quietly, without convulsions; but in many instances the animal has tetanic spasms and opisthotonos immediately preceding death, having symptoms somewhat allied to those produced by strychnia. Theine differs from strychnia in this respect:—In the latter there is increased reflex action of the cord, so that the slightest touch induces a tetanic spasm. In the former, there is no such excitability, as the posterior columns being paralyzed, and the animal not feeling any irritation, no spasm is induced, but convulsions occur without a stimulant.

EXPERIMENT IV.— $\frac{1}{4}$ gr. Theine. Frog. Ligature of Femoral Artery. Death. P.M.

The left femoral artery of a healthy middle-sized frog was tied, and $\frac{1}{4}$ gr. of theine, dissolved in 35 minims of water, was injected under the skin over the back. In ten minutes the animal was almost prostrate. It lay on its back, but was still able to contract its limbs when they were pinched, which both did with equal strength, the left leg perhaps being somewhat more sluggish than the right. Fifteen minutes later the frog was apparently dead. The head was amputated. On irritating the upper portion of the cord with the electrodes of a weak Faradic current, the two limbs contracted powerfully, and apparently with equal strength. When the sciatic nerves were dissected out and irritated or pinched, similar muscular contractions took place in both legs. The other symptoms and post-mortem phenomena were similar to those described in Experiment II.

Commentary on Experiment IV.—This experiment supports for-

mer observations in proving that the peripheral motor nerves are unaffected by theine. In tying the artery of the limb, the poisoned blood is prevented from circulating through it; consequently, while the rest of the body is affected, it remains in comparatively a healthy condition. After the introduction of the drug, the animal gradually became prostrate; but at no stage of the symptoms was there any special or marked difference between the two legs. (It may be mentioned, that cutting off the supply of blood to the limb produced slight weakness of that leg.) On irritating the spinal cord or the sciatic nerves, no difference was observed in the contractile powers of either of the posterior extremities. This proves that the peripheral motor nerves are not paralyzed; otherwise, the leg which had been poisoned would on irritation have been more sluggish in its movements than the limb from which the theine had been excluded by the ligature of the artery. In reality, the latter leg was the feebler of the two. This experiment shows nothing with regard to the sensory peripheral nerves, as, the posterior columns of the cord being affected, reflex action is destroyed.

EXPERIMENT V.— $\frac{1}{4}$ gr. Theine. Frog. Ligature of the Heart.

The heart of a healthy middle-sized frog was exposed by carefully dividing the sternum with a pair of scissors, and a ligature passed round its base, and tied so as to interrupt the circulation. The $\frac{1}{4}$ th grain of theine, dissolved in 10 minims of water, was then injected under the skin of the calf of the right leg. In four minutes both of the posterior extremities were partially paralyzed. On pinching the skin over the right calf with a pair of forceps, no muscular contractions of any kind followed. On pinching the skin over the left calf there were contractions of all the muscles of the body, including the right leg. On pinching any other portion of the skin, except the right leg below the knee, the animal struggled. A $\frac{1}{4}$ th grain theine, in 10 minims of water, was then injected under the skin of the calf of the left leg. Three minutes afterwards, when the toe of this limb was pinched, the muscles did not contract; but on pinching either of the anterior extremities, all four legs contracted.

Commentary on Experiment V.—In the former experiments it has been pointed out that theine paralyzes the posterior or sensory columns of the cord, but that it does not affect the motor filaments of the brain or cord, the motor nerves, or the muscles. The object of this experiment was to determine the local effect of theine upon the peripheral sensory nerves. The heart was tied so as to prevent any circulation of blood in the body. The operation produced partial paralysis of the limbs, probably owing to the want of proper blood supply. When the drug was injected into the calf of the right leg, subsequent irritation of the foot did not induce muscular contraction of that limb, but on irritating the toe of the left leg, there were distinct contractions of all the limbs, including the poisoned

one, showing that reflex action was unimpaired, and that the motor nerves of the poisoned leg were unaffected.

This supports the preceding experiment in proving that the nerves of the motor system are not paralyzed by theine, otherwise a pinch of the left leg (unpoisoned) would not have produced contraction of the right (poisoned). It also proves that the peripheral sensory nerves are paralyzed, as, in this instance, the cord not being under the influence of the drug, the loss of motion of the right leg was due to the local effect on the sensory nerves. This was further shown by injecting theine into the left or hitherto unpoisoned limb. A few minutes afterwards, when it was pinched, the previous muscular contractions were found to have disappeared. The sensory peripheral nerves had been paralyzed. When either of the anterior extremities were pinched, the muscles of the entire body, including both poisoned legs, contracted, showing that, although the sensory nerves of both limbs were paralyzed, the motor nerves were intact. Thus, theine paralyzes the posterior columns of the cord and the peripheral sensory nerves, but it does not affect the anterior columns or motor nerves.

EXPERIMENT V.—6 grains Theine. Rabbit. Death. Post-mortem.

Six grains of theine, dissolved in 2 drachms of water, were injected under the skin over the back of a healthy white rabbit, weighing 2 lbs. 3 oz. Almost immediately afterwards the ears were observed to become paler than before, then suddenly they appeared of a bright red colour, all the vessels being enlarged and congested. After remaining in this condition for half a minute, they again became pale and anæmic. These sudden changes from extreme pallor to intense congestion alternated for about five minutes, each stage being about a quarter of a minute in length, after which time the ears became permanently red, hot, and congested. The animal then became restless and somewhat excited, but not hyperæsthetic, and it trembled slightly. When its toe or ear was pinched, it struggled. The force of the heart's pulsations was stronger, and the rapidity of the beats, as well as the respiratory acts, quicker than before. Pupils were unaffected. Three minutes later, the hind legs straggled slightly, and they seemed to have lost power. Two minutes after, all four extremities were considerably weaker, and the rabbit was unable to stand upright, but lay flat on its belly, with all its limbs stretched out on the table. When its toe was pinched it did not struggle so much as formerly, still it pulled away its leg and attempted to crawl along, which it did in a shaky and laboured manner. The breathing was laboured and slow, the heart's pulsations were feeble, and the animal trembled. The ears were still intensely hot to the touch, and congested. Eight minutes later the animal could not make the slightest effort to stand, or move its body by means of its legs. All excitement had disappeared, and it lay quiet on its side. On pinching its toe very hard, it did not give

any evidence of feeling pain, and did not move. The ears were still congested and hot, and the pupils were somewhat contracted. For twenty minutes the animal lay in this prostrate condition, breathing in a laboured manner, when it suddenly took a tetanic spasm, with slight opisthotonos, which lasted for about a quarter of a minute. The congestion of the ears had now almost entirely disappeared. For the next seven minutes the animal took tetanic spasms at intervals, occurring spontaneously, and not brought on by pinching or other external irritations. Evidence of sensibility had disappeared from all parts of the body except the head, where it seemed to be normal. The eyelids winked when the eyeballs were touched, and even when the hands were clapped before them. When any portion of the face was touched, its muscles contracted. The animal, although completely paralyzed in its limbs, looked intelligent, as if sensation was unaffected. Ten minutes later the pupils were considerably contracted, the breathing slow and irregular, and the heart-beats not palpable. It died after a tetanic spasm.

An examination of the body was made as soon after death as possible. On applying the electrodes of a weak Faradic current to the exposed spinal cord, the muscles of the lower limbs contracted. On removing with a pair of scissors the bodies of the vertebræ, and isolating about half an inch of the cord, it was observed that, although muscular contractions followed irritation of both the anterior and posterior columns, these were much more marked in the case of the former. On irritating the brain, the muscles of the face and eye contracted. The sciatic nerves and muscles were also found apparently in normal condition. The membranes of the brain were much congested, but its substance appeared healthy, as was also the spinal cord. All the internal viscera were deeply congested, their vessels being engorged with blood.

Commentary on Experiment V.—In this experiment, the loss of motor power and other symptoms, already described in the mouse, ensued. The early stage was indicated by cerebral excitement, irritation, and restlessness of the animal, which subsequently disappeared, and was followed by depression and insensibility to irritation. It is to be observed, however, that the animal never seemed to lose its intelligence. It was watchful, followed every motion of the experimenter, although it was unable to make any movement. From the effects observed in the bloodvessels of the ear, it is evident that theine affects the vaso-motor nerves, first by irritating and afterwards by paralyzing them, as evidenced by the change in the action of the heart and the frequency of its beats, also in the tonicity of the smaller vessels by first contracting and lastly by relaxing them. This same action also explains the increase and subsequent diminution of the respirations which ultimately caused the death of the animal.

EXPERIMENT VI.—6 grs. Theine. Cat. Death. Post-mortem.

Six grains of theine, dissolved in $1\frac{1}{2}$ drs. of water, were injected

under the skin over the back of a healthy cat, weighing 4 lbs. 1 oz. In ten minutes, the animal became very angry and irritable. Fifteen minutes later, this excitement had increased, the animal had a watchful anxious appearance, prowled about, and when touched with a stick, bit at it and growled. If any noise or motion was made, it put up its back and made a hissing noise. The legs appeared weakened, and although it could still walk about, it preferred sitting in a corner of the room. Its mouth and tongue were very red, and there was an abundant secretion of saliva, which constantly trickled out of its mouth. The cat defæcated and mic-turated several times. Forty minutes later, it continued in much the same condition. Salivation was profuse. Animal suffered from tenesmus, and it had a constant straining from the bowel of a clear fluid, like mucus. The limbs, especially the posterior ones, were much weakened, but the animal could still run with difficulty. It could not jump; it made attempts to do so over a bench about two feet high, but failed. The breathing was laboured and irregular. The redness of the tongue and mouth, as well as the excessive irritability of the animal, had disappeared. It was quiet, lay in a corner, stupid and drowsy. It drank freely of water. Twenty minutes later, it was prostrate and lay on its side, its limbs quite helpless. It paid no attention to a pinch of the toe or a blow on the tail with a stick. It seemed, however, to be intelligent, as its eyes watched every movement of the observer, and when the hands were clapped before its face it growled. The salivation and discharge from the bowel were excessive. Pupils were contracted, and the breathing was laboured. Five minutes later, the cat took a series of tetanic spasms, and shortly afterwards died.

The post-mortem examination and phenonema were similar to those in the preceding experiment.

Commentary on Experiment VI.—In the cat the general nervous symptoms are much the same as in the rabbit, with this exception, that the cerebral excitement and subsequent depression are much more marked in the former. There was no evidence of hyperæsthesia, but rather a gradual loss of sensibility. In addition, there was excessive increase of the salivary secretion, and straining from the bowels of a profuse mucous discharge. The cat died after a severe tetanic spasm.

EXPERIMENT VII.—12 *grs. Theine. Rabbit. Spinal Cord exposed during life.*

A healthy white rabbit, weighing 2 lbs. 2 oz., was carefully fastened down on its belly. An incision was made through the skin along the upper part of the spine, about two inches in length, and the vertebral column exposed. By means of bone-forceps and scissors, portions of the vertebræ were removed so as to expose a piece of the spinal cord about a quarter of an inch in length. On touching the posterior column with the point of a blunt needle, the animal struggled violently and uttered loud cries. Twelve grains

of theine, dissolved in two drachms of water, were then injected under the skin of the belly. In ten minutes, the symptoms already described in preceding experiments commenced—congestion of the ears, etc. On pinching the toe, the animal did not appear to feel it. On touching with the point of a blunt needle the posterior columns of the cord, the animal struggled, but not nearly so violently as before, and it did not cry out. When the anterior columns were touched, there were violent convulsions of the body. Five minutes later the animal was completely paralyzed in all its limbs, and presented all the usual symptoms of prostration. A fresh portion of the cord was exposed by cutting away some of the vertebræ below the original wound. On touching as before the posterior columns, the rabbit only quivered slightly. On touching the anterior columns, marked muscular contractions of the limbs followed. The animal was shortly afterwards killed, and similar phenonema were observed after death as have been already described.

Commentary on Experiment VII.—The object of this experiment was to confirm, during life, what had already been concluded from post-mortem examinations, viz., that theine paralyzes the posterior columns of the spinal cord, while the anterior columns remain apparently unaffected.

These, and other experiments, have frequently been repeated, with various modifications and with different doses of the drug, so as to secure accuracy. Special observations were made to determine the effects of theine, caffeine, etc., upon the heart's action, the respirations, and the temperature, *1st*, by observing and noting the normal condition; and, *2dly*, by poisoning the animal, and ascertaining any changes in these functions which ensued. As a result of the whole inquiry, the conclusions already given at a former part of this paper have been arrived at.

In conclusion, I have only to state, that these actions of theine, caffeine, guaranine, cocaine, and theobromine are important as well as instructive. It is extremely interesting to learn, that those agents which the different nations of the world have found by experience to produce refreshing and stimulating beverages, although unlike one another, and procured from totally different sources, possess in common, proximate principles, which not only are almost identical in chemical composition, but also appear similar in physiological action.

That the effects of the beverages themselves are mainly if not entirely due to the neutral principles they contain, is highly probable, and there is every reason to believe that these concentrated forms of the different drugs will prove powerful and useful agents in the hands of the physician for the treatment of disease. The research, however, is yet in its infancy; and this contribution, I trust, may be looked upon as a stepping-stone to further inquiry. I venture to hope that my conclusions will not be found deficient in interest and importance to those who desire to establish a sound system of therapeutics upon careful physiological experiment.

Tables showing the Result of Seventy-two Experiments with Theine, Caffeine, Guaranine, and Cocaine.

TABLE I.—*Experiments with Theine.*

No. of Experiment.	Animal.	Dose in grs.	General Effects.	Result.	Post-mortem Examination.
1.	Frog	$\frac{1}{10}$	Slight weakness of posterior extremities.	Recovery	
2.	Frog	$\frac{1}{10}$	Weakness of posterior extremities.	Recovery	
3.	Frog	$\frac{1}{10}$	Paralysis of limbs; loss of reflex action; respiration impeded.	Recovery	
4.	Frog	$\frac{1}{10}$	Complete paralysis of limbs; loss of reflex action; respiration impeded; cutaneous congestion.	Recovery	
5.	Frog	$\frac{1}{10}$	Gradual prostration; paralysis of all the muscles; loss of reflex action; respiration stopped; congestion of cutaneous surface, mucous membrane of mouth, and tongue.	Recovery	
6.	Frog	$\frac{1}{10}$	Femoral artery tied; same effect as in No. 5; no difference between two legs.	Recovery	
7.	Frog	$\frac{1}{10}$	Complete prostration; loss of reflex action; stoppage of respiration; cutaneous congestion; stasis of blood in capillaries.	Death	Heart still beats; congestion of viscera; muscles contract when electricity is applied; when spinal cord, nerves, or brain are irritated, muscular contractions occur.
8.	Frog	$\frac{1}{10}$	Complete prostration; loss of reflex action; stoppage of respiration; congestion of cutaneous surface.	Recovery	
9.	Frog	$\frac{1}{10}$	The same as No. 8.	Death	Same as No. 7. Spinal cord exposed; anterior column irritated, contraction of muscles ensue; posterior column irritated, no contractions.
10.	Frog	$\frac{1}{10}$	Same as No. 8, but more rapid.	Death	Same as No. 7.
11.	Frog	$\frac{1}{10}$	Femoral artery tied; symptoms same as No. 8, but tetanic spasms in addition; no difference between two legs.	Death	Same as No. 7. No difference between two legs when electricity is applied.
12.	Frog	$\frac{1}{10}$	Rapid prostration, with tetanic symptoms.	Death	Same as No. 7.
13.	Frog	$\frac{1}{10}$	Upper part of spinal cord exposed during life; rapid prostration; after death, on touching anterior columns of cord, contraction of all the muscles; on touching posterior columns, no contractions; before death, muscular contractions followed irritation of the posterior columns.	Death	Same as No. 7.
14.	Frog	$\frac{1}{10}$	Lower portion of cord exposed during life; same as No. 13.	Death	Same as No. 7.
15.	Frog	$\frac{1}{10}$	Very rapid prostration and loss of reflex action, with usual effects.	Death	Same as No. 7.
16.	Frog	1	Almost instantaneous prostration.	Death	Same as No. 7.
17.	Rabbit	1+1	No effects from first grain. Slight weakness of posterior extremities from second grain.	Recovery	

TABLE I.—*Experiments with Theine—continued.*

No. of Experiment.	Animal.	Dose in gra.	General Effects.	Result.	Post-mortem Examination.
18.	Rabbit	1	Femoral artery tied; contraction of pupil very slight; no other effect; leg of animal was paralyzed by operation.	Recovery	
19.	Rabbit	2+2 +2+2	From first and second dose, no effects. From third dose, ears hot and congested; no other effect. From fourth dose, animal paralyzed and prostrate; tetanic spasms; contraction of pupil.	Death	Membranes of brain congested; substance of brain and spinal cord healthy; internal viscera congested; muscles contract when electrodes of Faradic current are applied; when applied to nerves or spinal cord, muscular contractions ensued.
20.	Cat	4+4	From first dose, great irritation and cerebral excitement; depression afterwards. After second dose, excessive salivation; partial paralysis of posterior limbs; tetanic spasms; tongue and mouth congested; mucous discharge from bowel.	Death	Nerves found sensitive to pinching. The same as No. 19.
21.	Rabbit	6	Observations on respiration—number, 1st increased, and 2d diminished. Complete paralysis of all four limbs; laboured breathing; loss of reflex action; congestion and heat of ears; pupils contracted; tetanic convulsions.	Death	Same as No. 19.
22.	Rabbit	6	Observations on heart's pulsation—number, 1st increased, and 2d diminished; otherwise same as No. 21.	Death	Same as No. 19.
23.	Cat	6	Irritable at first; staggering gait; excessive salivation; discharge of mucus from bowel; subsequent depression.	Recovery	
24.	Cat	6+3	From first dose irritation and cerebral excitement; embarrassed respiration; subsequent depression; profuse salivation; staggering gait; animal stupid and drowsy. From second dose, mucous discharge from bowel; mouth and tongue congested; vomiting.	Death	Same as No. 19.
25.	Rabbit	8	Ears congested; staggering gait; pupils contracted; laboured breathing.	Recovery	
26.	Rabbit	8	Ears congested; pupils contracted; paralysis of legs; reflex action lost except in head; tetanic spasms; opisthotonos.	Death	Same as No. 19.
27.	Cat	12	Spinal cord exposed during life; posterior column touched, animal cries and struggles. After injection of theine, complete prostration and loss of reflex action; on touching posterior column it does not move away or cry; on touching anterior column, muscles are contracted.	Death	Same as No. 19.

TABLE II.—*Experiments with Caffeine.*

No. of Experiment.	Animal.	Dose in grs.	General Effects.	Result.	Post-mortem Examination.
28.	Frog	⅞	Very slight weakness of posterior extremities.	Recovery	
29.	Frog	⅞	Slight weakness of posterior extremities.	Recovery	
30.	Frog	⅞	Considerable weakness of limbs; respiration impaired; reflex action diminished.	Recovery	
31.	Frog	⅞	Almost complete paralysis; respiration stopped; almost complete loss of reflex action; prostration; cutaneous surface congested.	Recovery	
32.	Frog	⅞	Complete prostration; respiration stopped; reflex action lost; under surface of skin, tongue and mouth congested; stasis of blood in capillaries.	Death	Heart beats feebly; great congestion of viscera. When brain is irritated, muscles of face contract; on irritating spinal cord, nerves, or muscles, there are muscular contractions.
33.	Frog	⅞	Ligature of femoral artery. Symptoms same as No. 32. No difference between the two limbs.	Death	Same as No. 32. No difference between the two legs.
34.	Frog	⅞	Spinal cord exposed during life. Symptoms the same as No. 32. On touching anterior or posterior columns with point of needle, strong muscular contractions followed. After caffeine was injected, no contractions followed touching the posterior column, while anterior column remained as before.	Death	Same as No. 32.
35.	Frog	½	Prostration and loss of reflex action; congestion of skin; stoppage of respiration.	Death	Same as No. 32.
36.	Frog	½	Rapid prostration and loss of reflex action, with usual symptoms.	Death	Same as No. 32.
37.	Frog	1	Ligature of femoral artery; very rapid prostration and loss of reflex action. No difference between the two limbs.	Death	Same as No. 32. No difference between the two legs.
38.	Rabbit	1+1 +1+1	From first and second dose, no effects. From third dose, congestion of ears, cerebral excitement, contraction of pupil, staggering gait. From fourth dose, partial paralysis of limbs, reflex action diminished.	Recovery	
39.	Rabbit	4	Numbers of the respirations and pulsations of the heart—1st increased, and 2d diminished. Temperature of the ear—1st diminished, and 2d increased. Ears at first anemic, subsequently hyperemic; breathing laboured; pupils contracted; paralysis of limbs; loss of reflex action; tetanic spasms; opisthotonos.	Death	Membranes of brain and internal viscera congested; substance of brain and spinal cord healthy; electrodes of current applied to cord, nerves, brain, or muscles, produced muscular contractions.
40.	Cat	6	Irritation and cerebral excitement; mouth and tongue congested; staggering gait; tenesmus and mucous discharge from bowels; salivation excessive; subsequent depression.	Recovery	

TABLE II.—*Experiments with Caffeine—continued.*

No. of Experiment.	Animal.	Dose in grs.	General Effects.	Result.	Post-mortem Examination.
41.	Cat	8	Same as No. 40; in addition, prostration, loss of reflex action, and tetanic spasms.	Death	Heart beats feebly. The same as No. 39.
42.	Cat	8	No marked effect for about half an hour. Death from sudden tetanic spasm.	Death	Same as No. 39.
43.	Rabbit	12	Spinal cord exposed during life; posterior columns, when touched with point of needle, animal struggles and cries out; anterior columns touched, animal struggles. After injection of caffeine, when posterior columns are touched, animal does not cry out, but struggles slightly; when anterior columns are touched, strong muscular contractions ensue. Symptoms same as No. 39.	Death	Same as No. 39.

TABLE III.—*Experiments with Guaranine.*

44.	Frog	$\frac{1}{16}$	No apparent effects.	Recovery	
45.	Frog	$\frac{1}{8}$	Animal sickly; slight weakness of posterior extremities.	Recovery	
46.	Frog	$\frac{1}{4}$	Weakness of limbs.	Recovery	
47.	Frog	$\frac{1}{2}$	Partial paralysis of limbs; reflex action impaired; respiration impeded; congestion of cutaneous surface.	Recovery	
48.	Frog	$\frac{1}{2}$	Prostration; almost entire loss of reflex action; respiration stopped; congestion of cutaneous surface.	Recovery	
49.	Frog	$\frac{1}{2}$	Complete prostration and loss of reflex action; respiration stopped; cutaneous congestion also of mucous membrane of tongue and mouth; stasis of blood in capillaries.	Death	Heart beats feebly; congestion of skin and internal viscera. On irritating brain, spinal cord, nerves, or muscles, there are muscular contractions; anterior columns of cord irritated, strong contractions of limbs; posterior columns irritated, no contractions.
50.	Frog	$\frac{1}{2}$	Rapid prostration and loss of reflex action; otherwise the same as No. 49.	Death	Same as No. 49.
51.	Rabbit	4	Number of the respirations and pulsations of the heart—1st increased, and 2d diminished. Temperature of the ear—1st diminished, and 2d increased. Ears—1st anæmic, and 2d hyperæmic. 1st cerebral excitement, and 2d depression; paralysis of limbs; subsequently apparent recovery, but afterwards died in sudden tetanic convulsion.	Death	Membranes of brain and internal viscera congested. Substance of brain and spinal cord healthy. Electricity applied to brain, cord, nerves, or muscles, produces muscular contractions.
52.	Rabbit	4+2	From first dose, ears congested, and cerebral excitement. From second dose, complete paralysis of limbs; loss of reflex action; contraction of pupil; tetanic spasms, with opisthotonos.	Death	Same as No. 51.

TABLE III.—*Experiments with Guaranine—continued.*

No. of Experiment.	Animal.	Dose in grs.	General Effects.	Result.	Post-mortem Examination.
53.	Cat	6	Irritability and cerebral excitement; subsequent depression; partial paralysis of limbs; respiration impeded; tongue and mouth congested; tenesmus and discharge of mucus from bowel.	Recovery	
54.	Cat	6	Same as No. 53; in addition, tetanic spasms and opisthotonos.	Death	Same as No. 51.
55.	Rabbit	8	Exposure of spinal cord during life. On touching posterior columns with point of needle, animal struggles and cries out; on touching anterior columns animal struggles. Guaranine produced effects as in No. 51. On touching posterior columns animal does not struggle or cry out; on touching anterior columns muscular contractions of limbs ensue.	Death	Same as No. 51.

TABLE IV.—*Experiments with Cocaine.*

56.	Frog	✠	Slight weakness of limbs.	Recovery	
57.	Frog	✠	Weakness of limbs.	Recovery	
58.	Mouse	✠	Weakness of limbs; slight congestion of cutaneous surface.	Recovery	
59.	Frog	✠	Considerable weakness of limbs; respiration impeded.	Recovery	
60.	Mouse	✠	Gradual weakness of limbs, ending in complete paralysis; cutaneous congestion; loss of reflex action; prostration.	Death	Congestion of membranes of brain, skin, and viscera. When brain, spinal cord, nerves, or muscles are irritated with electricity, muscular contractions ensue. Posterior columns irritated, no contractions; anterior columns irritated, strong contractions.
61.	Frog	✠	Ligature of the heart; right calf of leg injected. After no movement on irritation, on pinching left calf movements of entire body, including right leg.	Death	
62.	Mouse	✠	Same as No. 60; in addition, tetanic spasms.	Death	Same as No. 60.
63.	Frog	✠	Gradual prostration; paralysis of limbs and loss of reflex action; subsequent recovery.	Recovery	
64.	Frog	✠	Weakness of limbs at first; paralysis of limbs after. Loss of reflex action; stoppage of respiration; congestion of skin; stasis of blood in capillaries.	Death	Same as No. 60.
65.	Frog	✠	Ligature of femoral artery; symptoms same as No. 64. No difference in two legs.	Death	Same as No. 60. No difference between two legs.
66.	Frog	✠	Observation on lower part of spinal cord, which was exposed. Same as No. 64.	Death	Same as No. 60.

TABLE IV.—*Experiments with Cocaine—continued.*

No. of Experiment	Animal.	Dose in grs.	General Effects.	Result.	Post-mortem Examination.
67.	Frog	$\frac{1}{16}$	Observations on upper part of spinal cord, which was exposed. Same as No. 64.	Death	Same as No. 60.
68.	Frog	$\frac{1}{16}$	Spinal cord exposed during life. Any part of cord touched with needle, strong muscular contractions. Other symptoms same as No. 60.	Death	Same as No. 60. When posterior columns are irritated with point of needle no muscular contractions; when anterior columns are irritated contractions ensue.
69.	Frog	$\frac{1}{8}$	Same as No. 64, but more rapid.	Death	Same as No. 60.
70.	Rabbit	$3 + \frac{1}{2} + \frac{1}{2} + \frac{1}{2}$ 3	From first dose, congestion of ears; from second dose, slight cerebral excitement; from third dose, great irritability, no loss of sensibility; from fourth dose, congestion of ears increased; from fifth dose, depression, staggering gait. Numbers of respirations and pulsations of the heart—1st increased, and 2d diminished. Temperature — 1st diminished, and 2d increased.	Recovery	--
71.	Frog	$\frac{1}{16}$	Same as No. 60. Number of respirations—1st increased, 2d diminished, and 3d stopped.	Death	Same as No. 60.
72.	Frog	$\frac{1}{16}$	Same as 60. Pulsations of the heart— 1st increased, 2d diminished, and 3d stopped.	Death	Same as No. 60.

Part Second.

REVIEWS.

The Influence of the Mind upon the Body. By DR HACK TUKE.
London: J. and A. Churchill.

UNDER the above title, Dr Hack Tuke has published an account of various individualities wherein certain "mental" influences have greatly modified, or altogether suspended, the operations of the general organism.

Dr Tuke's book is arranged as follows:—

Introductory remarks. *The Intellect* (that portion of the subject which relates to "Intellect"—of what it consists—its influence upon sensation—muscular contraction—and the organic functions). *The Emotions* and their influence discussed in a similar order. *The Will* and its influence discussed in a similar order.

We would now shortly reflect upon Dr Tuke's account of the "Influence of Mind upon Body;" but before doing so, would fain suggest the difficulty of Dr Tuke's task, and our own consequent consciousness of diffidence in handling such a question, though only in the humble character of reviewer. Let us try to come at some sort of approximative idea of what we mean when we speak of "man." The influence of mind upon body is interesting, chiefly because "mind" and "body" are attributes of "man." What is man?

Dr Tuke approaches his subject in virtue of one of the abilities of "man"—his ability as an observer. It is in virtue of this ability that we would now speak.

Man as we see him—as each of us here lives, moves, and has his being—is doubtless, even to the outward eye, the most wonderful creation of which we have knowledge—an embodied existence which performs numerous exploits affecting the existence of others—his fellows. Man is affected by numberless impressions, which induce varied effects in him; may lead to varied modes of action by him; like impressions may affect different men in ways which differ from each other through an endless range. What is very curious, not the least vivid impression reaching a man fails to produce *some* effect on him, some change *in his organism at least*; and if the matter be looked curiously into, it will be found that every little change in every individual can be traced to produce effects of greater or less gravity in the individuals of his special community, again *at least*.

This creature "man" is capable of entering into certain relations with other members of the species, by which the existence of the species is continued. And in the case of each, the creature passes through certain stages of growth, maturity, and decay—adolescence, manhood, and old age; after an existence of threescore years or so, "vitality" is lost; dissolution ensues; the various phenomena of life cease from our beholding; and the creature, as cognisable to our present powers of observation, becomes "inanimate."

We may then safely state that man is a creature who, to our knowledge, and patently to our powers of observation, performs various functions of a highly interesting complexity, and of a reconditeness in even the mechanism for their production which has at least hitherto baffled all investigation.

The study of "man" has always possessed for man peculiar attraction, and each student has approached his subject by the path most congenial to his tread. Some have devoted laborious and useful lives to the consideration of the mechanism of that organism by means of which the various phenomena of human life are elaborated. Some have bent themselves with their utmost ability to gain some glimmering as to the recondite "why" of these phenomena, the "whence" of the capacity for their appearance, and the "wherefore" of their appearance at all.

Dr Tuke in his introduction says, "The terms chosen for the title of this work accord with popular usage, and are probably less like to mislead than any others which might be used; but the only satisfactory mode of stating the case would be one founded upon a correct and complete physiology of the brain, or rather of the entire nervous system."—With this we cannot agree; to suppose that all phenomena of mind and all mental manifestation would be explained by a physiology however perfect, would be to hold man a mere cunning piece of mechanism: a machine which, with certain modifications, is found to grind philosophies, arts, morals, etc. Doubtless the plan of describing phenomena of organic action by the use of terms held in popular usage to define certain so-called mental conditions is a wise one. Truly, "Great, however, as has been the advance of late years in the knowledge of the functions of this" (the nervous) "system, it does not enable us to speak with perfect physiological precision on all points; and it is more than probable, that no amount of scientific knowledge will ever displace the time-honoured phrases of 'mind' and 'body.'" Indeed no—the physiologist is in fact a mechanist: he makes the *machinery* of vital action his study; but never will his laborious research enable him to tell how it is, that of two brains presenting identical elements to every appliance for investigation at his command, the one is that of a man possessed of capacity qualifying him as a Heaven-sent prophet to his fellows; the other that of a glutton and sluggard, whose sole aim it is to pamper the grossness of his earth-chained soul. The physiologist can describe to a certain extent in detail *the tracts* by which impression reaches the brain of man, and the general design of the mechanism through which, in all probability, the actions resulting from such impressions occur; but he cannot inform us why identical impressions arouse laughter in one, sorrow in a second; why one man will look on some scene of another's misery and be filled with a godlike pity and desire to relieve, while another will make brutal jest of the distresses of a Lazarus, whose aid he would be the first to implore were their cases reversed. The man of science cannot, in virtue of science, instruct us as to the springs of capacity which enabled Socrates, from a study of his own existence, and the other existences in the nature of God surrounding him, to gain his intuition of a future state of existence—to form his doctrine of a future state of being connected with, blossoming from, the conduct of the present, and approaching in sublimity the standard of Another's teaching held by us as Divine. What were the various influences brought to bear on Socrates during his life conduct, carried on by means of an organism no doubt elementally indistinguishable from those of his less enlightened compeers, that he gained a glimpse of *immortality*, and cheerfully quitted this palpable existence in the sure faith of enjoying a better? What were the influences which moulded such lofty beliefs in this man, and how was it they did not and do not visit and produce like results in others?

The morbid anatomist can demonstrate that by pressure on great nervous trunks, or approximatively recondite changes in nervous structure, different parts of the human body become incompetent to perform their functions; he may say the nervous influence of the brain or other centres is thereby interfered with. But what is that nervous influence? Where and how are generated those vital forces which give vigour to bodily action, clothe the cheek with a healthy glow, nerve the brain for intellectual effort? And what is gone when "*life*" departs, and that once energetic body and active brain remain a wonderful but powerless congeries of organic appliances?

The function of "science" is that of the man with the lamp, whose duty it is to search diligently into the *appliances* by means of which that wonderful piece we call life is played by the various actors in it: the mechanism is wonderful: the science which strives towards further elucidation of its mystery is one of the most dignified of the many forms of inquiry; but the nature of the form and mode of action of organs is the subject of its research. Within the fleshly parts, there is an immortal spirit at work, and this appearance which we call man is only man's "time-vesture," in which he works through this existence, which is but a phase of his being. "These limbs, whence had we them; this stormy Force; this life-blood with its Burning Passion? They are dust and shadow; a shadow-system gathered around our ME; wherein, through some moments or years, the Divine Essence is to be revealed in the Flesh."¹

To us it appears that a thick mist must always surround such an inquiry as the present; such is the delicacy and exquisite elaboration of that organism with which man is endowed, that, even as regards elementary inquiry, it becomes a matter of well-nigh impossibility for the most subtle investigator into the springs of the actions of another to decide when such action is a deliberate result regulated by that individual's judgment, and when the result of that automatic action which is a property of his organism, and must be considered as more belonging to the mechanical part of the "man."

We would very shortly glance at the Excito-motory action of the brain and nervous system. This property of vitalized nervous matter seems to us intimately connected with many of the instances given by Dr Tuke, where "intellect" and "emotion" evidence their influence by extraordinary action of the general organism.

A man of nervous, that is, impressible temperament. If such a man's foot be tickled, he will *involuntarily* kick out; if such a man hears a bullet whiz past his ear, he will *involuntarily* start aside; if such a man see a child about to be cut down by a railway train, he may involuntarily make involuntary motions of distress, or utter involuntary cries of horror from apprehension of the catastrophe which he sees about to occur, but be unable to take any voluntary action for the prevention of such. A man of highly susceptible

¹ Mr Carlyle, *Sartor Resartus*, p. 184.

mind, but of corresponding controlling power, hears that in a burning house a helpless child has, in the panic-stricken rush out, been forgotten and left to perish; the terror and agonies of the deserted being, the difficulties of its rescue, crowd upon his mind; but the man has his organism in hand, so to speak, and the terror of the situation wholesomely serves to spur the mental machine into full activity; instantly, with cool promptitude, he organizes in his mind a scheme of rescue, and with corresponding bodily activity proceeds to carry such a scheme into effect.

It will be noticed that in the above, instances are presented where, in the case of a supposed man of less than ordinarily well-balanced mind, different impressions are supposed to produce excitatory effects in different parts of his organism. Results such as the first are shared by him with the lower forms of animal life in which the ganglionic system is developed. Results such as the second are shared by him with all animals possessing more highly developed sensory ganglia. Results such as the third are peculiar to man alone; he owes his inability to take useful action in such a juncture simply to the great elaboration of his organism. Results such as the fourth are peculiar to the comparatively sane man alone.

The constitution of the organism of men varies infinitely as regards all their parts; as regards the arrangement of the parts of the nervous system, with which at present we have to do, the constitution varies infinitely. No two men, when the matter is closely looked into, resemble each other closely, as regards what may be called the intellectual powers—as regards the general nervous vigour—as regards the vigour of special parts of the nervous system—as regards the mode and degree in which one part of the nervous system influences the remainder or other special parts—as regards the mode and extent to which the operation of the nervous system of one person is liable to be influenced by the operation of the nervous system of another—as regards the interdependence of the nervous system and the rest of the organism. But though no two men resemble each other closely, they resemble each other generally; and the differences are differences in degree, not in kind. Again, the nervous organization of any given human being is a link in a chain of organisms, and possesses relations to its forebears in a family series; but the relation is of a complexity which, for its complete elaboration, must, we fear, be confessed to baffle all research.

In certain phases of life, the nervous system gets into conditions in which its manifestations are abnormal to the usual constitution of the individual, though they cannot be considered the result of diseased states.

The vigour and nature of nervous ability may be greatly modified by impaired, or diseased, or disordered states of the whole organism or parts of it—of special parts of the nervous system.

We would then express our opinion that Dr Tuke's examples—those relating to the influence of the "intellect" and "emotions"

on organic activity—must be held as examples of the disordered action of the central and nervous machinery primarily; in many of these examples, the disordered action extended consequently to the general organism.

To the wearied reader who has borne with us thus far, we would say, a "little longer patience;" it seems impossible to us, without defining our own situation as to the matter, to describe the opinions we have formed of Dr Tuke's book.

We would fain now tersely summarize what we have written, fain condense the misty glimmerings of our feeble light, and define our fundamental belief in these matters. The reader will bear with our incompetency. It seems to us, that this time in which we dwell is occupied by what we will call existences, for want of a better term. In these existences, we believe to coexist in wonderful combination "the temporal" and "the eternal." Our belief is in an immortal soul, the son of the Highest, put here to a schooling by the Master, gifted for the purpose of this state of existence with a body through which he manifests himself in part to the temporal eye. The body is endued with an abstraction named its vitality, of the nature of which we know nothing, *except that it is*. Of this vitality, the body is liable to be deprived under various circumstances. It may be held that the nature of each man's organism is adapted to his requirements. But these things are very dim, for the strange phenomena arise of the spirit struggling with, and apparently being vanquished by, the vitalized organism; of organisms derived in the lineal series from other organisms, being tainted and warped by the congenital influences of apparently culpable methods of conduct of parents.

There are then, for the purposes of the present—(1.) Men who have not their organism, of whatever degree of capacity, under control. Many of the actions of such "intellectual" and "emotional" must be held as only excito-motory actions of a vitalized organism. There is no evidence of intelligent control. (2.) Men who have their organisms, of whatever degree of capacity, under control. We would now state that our opinion is that Dr Tuke's book is valuable as a collection of instances of what may be in the case of humanity. The individualities mentioned bear the same relation to what we may conceive as ordinary individuality that diagram does to finished drawing—we find features of the organism in its various relations exaggerated; not exaggerated in Dr Tuke's account of it, far be it from us so to speak,—but exaggerated naturally, doubtless, by Omniscience for the individual's needs. We receive such reverently; but would question if it is well, in the present state of knowledge, perhaps in any state of knowledge attainable by human intelligence, to found *conclusions* upon these; we may gather hints, but would be chary of founding conclusions.

Of the well-nigh impossibility of definition in handling such a

subject, we are convinced, and would refer to Dr Tuke's chapter on "Intellect" as evidence to this.

Dr Tuke states that under this term he includes "the use of the Intellectual powers generally." What are they? "Attention" is included under Intellect; but we should rather hold attention to denote the direction of the mental power by the indwelling spirit. "Ideas" are included under the Intellect. "The term Ideas has been employed in so many senses from the days of Plato, the reputed originator of the word, to the present time, that it will not be amiss to add to the foregoing a brief reference to some of the significations attached to it." Two and a half pages are devoted to an enumeration of the *different meanings* attached to a designation of a part of Intellect. So of Imagination, to a consideration of the different meanings of which term eleven pages are devoted; in these pages mental qualities and attributes most varying in point of importance are all held as expressed in this term, till it is finally found to be very closely allied to other "Intellectual" properties, as Imitation, etc.; and after this introductory chapter as to what the popular term "Intellect" means, the bewildered reader is told that the cases which follow are instances of the influence of "Intellect" upon Sensation, upon the voluntary and involuntary muscles, and on the organic functions!!

We would submit that "Intellect" is a comprehensive term impossible of "definition," only possible of inquiry into, used to denote the more or less perfect working of the whole intellectual machine; it includes also the idea of "the man" himself being *deliberately* concerned in such working.

The influence of the Emotions on organic action.—Dr Tuke confesses a rather strong leaning to any physiology of the emotions which "recognises a special and intimate relationship between them and the medulla oblongata, or some portion of the sensory ganglia;" "that while the various nervous centres which compose the base of the brain are the *conductors of voluntary motor influences*, they are the centres of emotional movements." Lockhart Clarke is also quoted as regarding it possible that the power of expressing emotions and desires is dependent upon the co-ordinating functions of the olivary bodies. Schroeder Van der Kolk is quoted as going a step further, as finding that the olivary bodies are more highly developed in carnivorous than in herbivorous animals. He says: "The superior corpora olivaria appear to be organs for the involuntary or reflex expression of the passions." "In birds they serve for the movements of the feathers on the head and neck in passion." But all this does not go in the least to prove that in the sensory ganglia the emotions have their seat. An emotion is a mental phenomenon which arises on receipt of an impression. The production of an emotion involves action in some part of the mental machine. In extremely susceptible brains, or where the impression is overwhelmingly strong, or the character of a weak cast, other

parts of the cerebral mechanism may become involved, and various forms of organic action, patent to observation, of an *involuntary* kind, may occur. Consider as examples: (1.) The account given by Dr Tuke, p. 249, of Lucretia Davidson. "Her susceptibilities were so acute, and her perceptions of beauty so exquisite, as to cause her to faint when listening to some of her favourite melodies from Moore." The deficiency of control and judgment is here very noticeable. (2.) The frequently occurring case where a man's face and hands involuntarily twitch in the extremity of a terror, which renders him incapable of voluntary action. (3.) Consider the involuntary laughter and sobbing of an hysterical woman—this occasioned by the slightest impression, and quite beyond control. The sensory ganglia, no doubt, are frequently brought into action as the result of the occurrence of emotion; but we cannot see that they can on that account be considered the seat of such emotion.

Dr Maudsley seems to afford some grasp of the nature of "emotion" when he says: "Emotion is strictly perhaps the sensibility of the supreme centres (hemispherical ganglia) to ideas."

But in all the examples given by Dr Tuke of the influence of emotion on organic actions, we contend that emotion—the immediate rendering by the brain of an impression—plays a part of undue gravity. The function of emotion is to present certain impressions to the deliberative powers. When emotion initiates action it is acting as an unruly or unruléd servant, and is usurping an office for which it is neither fitted nor intended.

The influence of the Will upon organic action.—We shall quote two examples, which seem to us to embody the gist of "The influence of the will upon the body," and of "psycho-therapeutics."

At p. 382, we read as follows: "Whether and to what extent the physician may avail himself of fear in the treatment of disease, has often been discussed. It cannot be denied that while inflicting a great deal of suffering, it has been successful in not a few instances, as in the case of the lady that was cured of the vapours by a noble lord, who arranged that in the midst of one of her most violent fits, four mutes dressed in white should enter her apartment, slowly approaching, and take her, without violence, in their arms, and, *without giving her time to recollect* herself, convey her to a distant chamber, hung with black and lighted with green tapers." The italics are our own.—The case of a woman of an extremely susceptible organism; no controlling power; every means possible taken to nullify what little she had. The cure was but a change in the active mechanism, with which the *ego* had nothing to do. The further history of the above individual is not given.

Again, p. 386: "The influence of the will in controlling disease has already been incidentally referred to in the case of Irving. His own account of an attack of cholera may be made use of advantageously here. During the invasion of the cholera in 1832, he was seized with what was in all appearance, and to the conviction

of medical men when described to them, that disease which has proved fatal to many of our fellow-creatures. He had risen in perfect health at his usual early hour. By breakfast-time he had become very cold, and was labouring under severe pain. His appearance shocked his friends. Vomiting succeeded, and wringing or gnawing pains; and, being so weak that he could not sit up, he lay on the bed wrapped in blankets till he had to set out to preach, at half-past eleven. It appears that he had a little brandy and arrowroot, but felt no better. With sunken eyes, pallid cheeks, and an altogether ghastly appearance, he tottered to the church, a quarter of a mile distant, and found another minister officiating for him. He was tempted to shrink back, but summoned resolution to tell his beadle to go into the pulpit and inform him he would shortly take his place. In the meantime he stretched himself on three chairs in the vestry before the fire. 'Even as I shifted my position I endured much suffering, and was almost involuntarily impelled to draw up my limbs in order to keep the pain under. Nevertheless, when I stood up to attire myself for the pulpit, and went forward to ascend the pulpit stairs, the pain seemed to leave me.' His sight was dim, his head swam, he breathed with difficulty, he laid hold on the pulpit sides, and looked wistfully about, wondering what would befall him. The crisis came. 'That instant a cold sweat, chill as the hand of death, broke out all over my body, and stood in large drops upon my forehead and hands. From that moment I seemed to be strengthened.' He preached upwards of an hour, with more unction than he had ever done before. After the service he walked home, eating little or nothing. Yet he preached in the evening in a crowded schoolroom, and next morning rose before the sun, to pursue, 'with renewed strength,' what he regarded as his course of duty. The narrative, Mrs Oliphant suspects, may cause some to smile, but it is impossible not to admire so resolute though mistaken a man."

With a brief consideration of the above we will conclude. We will make this incident in the life of Irving our text, our natural illuminator of our view of the influence of "mind" upon "body." Of the above, we would suggest as follows: An eager, enthusiastic, powerful soul, endowed with a temporal vitalized mechanism thrilling to every influence adjudged to reach it. For long the life-conduct of this man was matter of wonder and admiration to many, reverential awe to some. He, by his wonderful eloquence, is said to have fascinated men of the most lofty consideration as regards intellectual power; and he founded a church whose members regarded him with an awful love, and guided their lives by his advice. In the above incident he was seized with cholera, or at least violent intestinal disturbance. Such perturbation of one part of the organism of course threatened to impair the capacity of the rest; and this man, who was in the constant habit of disciplining the unruliness of the mortal organism—the instinctive tendency of which was towards

wrong, and like the organism of every man required to be constantly corrected and kept right—by powerful disciplining and restraint, regulated the morbid action of even the more material parts of his organism. By willing it, he became well.

Once more, consider these two last sequent cases : the one a weak woman, who was frightened into a change of her disorder ; the other a strong man, who, in virtue of a constant, true exercise of the governing capacity, forced his organism back to that path of rectitude from which it had diverged. We gaze with wonder on such diversity of existence, of the outcome of which we know nothing. Truly, the Omniscience which knows *even this*, and the Omnipotence which ordains it, is to us but a name, and conveys no meaning, but only awe. These are but two cases from Dr Tuke's collection, and Dr Tuke's collection bears truly an infinitesimal relation to the All. The all consists of units, each differing to the measure of individual peculiarity each from each ; and when we try to take what one would call a comprehensive view, it is as if we gazed at the indistinguishable luminous specks which dance in the immeasurable sunlight of Omniscience.

The above episode in the life of Irving bears a certain relation to Dr Tuke's account of psycho-therapeutics. We know, each of us in a minor degree, the influence which man exercises on man, the more powerful on the weak. We hold that the whole organism is interdependent ; if one part of man's organism, the brain, can be so impressed by the influence of another as to regulate its mental function by his dicta, in spite of the efforts of that organism's proper master, we see nothing impossible in that influence extending to other parts of an interdependent machine, whose ruler is ruled by that "Another."

A Treatise on the Diseases of Infancy and Childhood. Second Edition, enlarged and thoroughly revised. By J. LEWIS SMITH, M.D., etc. London: Trübner and Co. Philadelphia: Henry C. Lea: 1872.

We are not sure that the author of this book may not come upon us for indirect claims, as a favourable notice in the *Edinburgh Medical Journal* must be of some value, and should not have been so long deferred.

Those, too, who are inclined to listen to our recommendation in the purchase of a good text-book, have also some reason to complain of our delay ; nevertheless, we are not willing to refer the matter to arbitration.

The work of Dr J. Lewis Smith is an octavo of 730 pages. The author is well read in the literature of the subject, whether English, French, or German. He has evidently had a ripe experience in the

study and treatment of the diseases of children, and his book forms a complete treatise on the subject.

There is no doubt that the difference in mode of life, and especially in climate, makes the treatment of disease to a certain degree different in the United States from what it is in our country; but then many of our medical graduates go abroad, and to climates similar to that of the United States. In any case the modifications required from difference of climate will be noticed by the discerning reader.

We cannot say that we have found anything to censure in a dogmatic form, nor do we wish to raise an argument upon points capable of dispute. Like many of his countrymen, the author is fond of Latinized expressions. He would rather say *acuminate* than *pointed*, or *palpation* than *touch*; *headache* frequently becomes *cephalalgia*; *bad weather* is *unpropitious weather*, and so on. It also strikes us that his sentences are sometimes awkwardly constructed. It would be better if they were shorter and more direct. The following, picked out almost at random, may be taken as an instance:—

“Physicians are so well aware of the importance of domiciliary cleanliness and ventilation, of the free admission into the nursery of solar light, and of the importance of out-door exercise as a means of invigorating the system and promoting healthy functional activity, that nothing need be stated in reference to these subjects in this connection.”

The book, however, is readable enough; the sense is generally clear, the language precise, and the statements carefully qualified.

The author has a very good chapter on Lactation. He thinks that the mother, in good health and furnishing an average quantity of milk, is competent to give all the nutriment which the infant requires until it has reached the age of four months, and most are until the age of six months.

He has a very unfavourable opinion of artificial feeding of the young infant, especially in large towns. “In New York,” he says, “the foundlings of the cities afford the most striking and convincing proofs of the advantage of lactation. In some cities foundlings are wet-nursed, while in others they are dry-nursed, and the result is always greatly in favour of the former. Thus, on the Continent, in Lyons and Parthenay, where foundlings are wet-nursed almost from the time that they are received, the deaths are 33·7 and 35 per cent. On the other hand, in Paris, Rheims, and Aix, where the foundlings are wholly dry-nursed, their deaths are 50·3, 63·9, and 80 per cent.

“In this city the foundlings, amounting to several hundreds a year, were, till recently, dry-nursed, and, incredible as it may appear, their mortality, with this mode of alimentation, nearly reached 100 per cent. Recently, wet-nurses have been employed, for a part of the foundlings, with a much more favourable result.”

The following is Dr Smith's opinion on a point which has raised much difference of opinion :—

"Sometimes women, prior to pregnancy, present indubitable evidence of tuberculosis, but by the improved general health which attends pregnancy, the disease is temporarily arrested. Such women should never suckle their infants. If they do, they soon lose all that was gained, and the disease advances rapidly."

The following passage about the treatment of enlarged scrofulous glands accords with our own experience :—"It is the common practice to treat these glands, if they are subcutaneous, by daily application over them of the officinal tincture, the compound tincture, or the compound ointment of iodine. It is my opinion, from observing the effects of these agents, that they are too irritating for ordinary cases. Applied daily, they cause proliferation of the cells of the epidermis, so that in two or three days the thickening of the cuticle is greatly increased, and its external layer begins to exfoliate. It has appeared to me that what we observe in the epidermis, illustrates, to a certain extent, what occurs in the gland underneath, as a result of active counter-irritation. The gland does not resolve, its superfluous cells are not destroyed and absorbed, as was desired, but the treatment tends rather to increase the proliferation of the cells of the gland or the formation in it of true leucocytes. We have seen that a local cutaneous inflammation, as eczema or impetigo, is apt to cause the neighbouring lymphatic glands to enlarge. How, therefore, can we expect to reduce a glandular swelling by a mode of treatment which establishes a similar condition? I once produced, partly by accident, such an amount of vesication over an enlarged, hard, and apparently somewhat indolent gland, in an infant of fourteen months, that for a week I was very anxious lest a sore would result, which would heal with difficulty, or leave a permanent cicatrix, and yet, instead of dispersion of the glandular swelling, the pathological processes were so promoted that suppuration and discharge of pus occurred by the time that the cuticle had re-formed. If hyperplasia of the lymphatic glands could be cured by counter-irritation, it should have been in this case.

"The correct mode of treating these glands, therefore, as regards external measures, I hold to be, to apply the iodine preparations in such a manner that the largest amount of iodine will reach the glands by absorption, with little irritation of the skin. I am not prepared to state what is the best formula for the application of this agent. During the last few months, we have been attempting to determine this in the children's class at the Out-door Department at Bellevue, but our statistics of cases are not at present sufficiently complete or numerous to enable me to make a positive statement. I feel justified, however, from the observations already made, in recommending the following formulæ, as preferable to the officinal preparations which are commonly employed :—

"R Potas. iodidi, 3j ;

Ung. stramonii, 3j ; Misce.

"To be rubbed over the gland several times daily. It should not be applied as a plaster, as it is too irritating and will vesicate. I have known a glandular swelling, which had continued about three months, to disappear in as many weeks, under its use in connection with internal remedies. Glycerine may be employed in place of stramonium ointment."

It is known that in America the scab is much used for vaccination instead of the lymph. This is very convenient, as the scab keeps much better. The author, while he prefers the scab to the lymph, if it be old, is of opinion that syphilis and erysipelas are now and then communicated through the scab when there would be no danger from the lymph.

Dr Smith, in his chapter on Cyanosis, has interested us much. He finds it much more common with boys than with girls. On the contrary, chorea is more common with female children.

"The emotions are strong in girls, since in them the nervous system predominates, while the muscular power is weaker than in boys. Hence a partial explanation of the fact which statistics fully establish, that the proportion of choreic boys to girls is about in the ratio of one to two and a fraction. I have remarked, in this city, the large proportion of cases in school girls between the ages of six and twelve years; the severe discipline and confinement of the public schools no doubt increasing the strength of the emotions, and weakening the control of the will over the muscles."

This is a higher proportion than has been noticed in our own country. We imagine that it would be easy to accumulate instances from this as from many other works upon disease in the United States, of the harm done to the health of females by the adoption of the views of theorists who wish to assimilate the education of women to that of men, and to make light of natural distinctions, which, however, cannot be evaded without moral disorder and physical disease.

The author treats Cretinism as hypertrophy of the brain. He adds: "A recent thorough examination of the subject lends support to the view that cretinism is caused by the use of water containing one of the combinations of sulphur and iron." We confess we have not heard of this important examination before. We may admit, in conclusion, that as we have kept the book long in our hands, we have several times consulted it in doubtful cases, and have generally obtained useful advice.

INFORMATION REGARDING MEDICAL EDUCATION AND EXAMINATIONS.

THE following are the Regulations of the General Medical Council on the subject of Medical Education and Examinations:—

I.—PRELIMINARY EXAMINATIONS.

1. That Testimonials of Proficiency, granted by the National Educational Bodies according to the subjoined list, may be accepted, the Council reserving the right to add to, or take from, the list.

List of Examining Bodies whose Examinations fulfil the Conditions of the Medical Council, as regards Preliminary Education.

(1.) UNIVERSITIES OF THE UNITED KINGDOM.

Oxford.—Examination for a Degree of Arts. Responsions, Moderations, Local Examinations (Senior), Certificate to include Latin and Mathematics.

Cambridge.—Examination for a Degree in Arts. Previous Examination. Local Examinations (Senior), Certificate to include Latin and Mathematics.

Durham.—Examination for a Degree in Arts. Examination for Students in their second and first years. Registration Examination for Medical Students. Local Examinations (Senior), Certificate to include Latin and Mathematics.

London.—Examination for a Degree in Arts. Matriculation Examination.

Aberdeen, Edinburgh, Glasgow, and St Andrews.—Examination for a Degree in Arts. Preliminary Examination for Graduation in Medicine or Surgery Examination of (Senior) Candidates for Honorary Certificates under the Local Examination of the University of Edinburgh.

Dublin.—Examination for a Degree in Arts. Entrance Examination.

Queen's University (Ireland).—Examination for a Degree in Arts. Entrance Examination. Examination for the Diploma of Licentiate in Arts. Previous Examination for B.A. Degree.

(2.) OTHER BODIES NAMED IN SCHEDULE (A) TO THE MEDICAL ACT.

Royal College of Surgeons of England.—Examination conducted under the Superintendence of the College of Surgeons, by the Board of Examiners of the Royal College of Preceptors.

The Society of Apothecaries of London.—Examination in Arts.

Royal Colleges of Physicians and Surgeons, Edinburgh.—Preliminary Examination in General Education, conducted by a Board appointed by these two Colleges combined.

Faculty of Physicians and Surgeons of Glasgow.—Preliminary Examination in General Literature.

Royal College of Surgeons in Ireland.—Preliminary Examination, Certificate to include Mathematics.

Apothecaries' Hall of Ireland.—Preliminary Examination in General Education.

(3.) EXAMINING BODIES, IN THE UNITED KINGDOM, NOT INCLUDED IN SCHEDULE (A) TO THE MEDICAL ACT.

Royal College of Preceptors.—Examination for a First Class Certificate.

(4.) COLONIAL AND FOREIGN UNIVERSITIES AND COLLEGES.

Universities of Calcutta, Madras, and Bombay.—Entrance Examination, Certificate to include Latin.

McGill College, Montreal.—Matriculation Examination.

University of Toronto, King's College, Toronto, Queen's College, Kingston, Victoria College, Upper Canada.—Matriculation Examination.

King's College, Nova Scotia.—Matriculation Examination. Responsions.

Dalhousie College and University, Halifax, Nova Scotia.—Matriculation Examination.

University of Fredericton, New Brunswick.—Matriculation Examination.

University of Melbourne.—Matriculation Examination, Certificate to include all the subjects required by the General Medical Council.

University of Sydney.—Matriculation Examination.

Codrington College, Barbadoes.—1. English Certificate for Students of two years' standing, specifying the subjects of Examination. 2. Latin Certificate, or "Testamur."

Tasmanian Council of Education.—Examination for the Degree of Associate of Arts, Certificate to include Latin and Mathematics.

Christ's College, Canterbury, New Zealand.—Voluntary Examination, Certificate to include all the subjects required by the General Medical Council.

The Examiners for Commissions in the Military and Naval Services of the United Kingdom.—Certificate to include all the subjects required by the General Medical Council.

Cape of Good Hope.—Third Class Certificate in Literature and Science granted by the Board of Public Examiners.

South Australia—South Australian Institute, Adelaide.—Preliminary General Examination.

N.B.—A Degree in Arts of any University of the United Kingdom or of the Colonies, or of such other Universities as may be from time to time recognised by the General Medical Council, will be considered a sufficient testimonial of proficiency.

2. That it be recommended to the Licensing Boards not to accept the Certificate of Proficiency in General (preliminary) Education from any of the Bodies, the names of which are contained in the list annually circulated, unless such Certificate testify that the Student to whom it has been granted has been examined in:—1. English Language, including Grammar and Composition.¹ 2. Arithmetic, including Vulgar and Decimal Fractions. Algebra, including Simple Equations. 3. Geometry—First two books of Euclid. 4. Latin, including Translation and Grammar.

And in one of the following optional subjects:—Greek. French. German. Natural Philosophy, including Mechanics, Hydrostatics, and Pneumatics.

3. That students who cannot produce any of the Testimonials referred to in the first Recommendation be required to pass an Examination in Arts, established by any of the Bodies named in Schedule (A) to the Medical Act, and approved by the General Medical Council.

II.—REGISTRATION OF MEDICAL STUDENTS.

1. Every Medical Student shall be registered in the manner prescribed by the General Medical Council.

2. No Medical Student shall be registered until he has passed a Preliminary Examination, as required by the General Medical Council.

3. The commencement of the course of Professional Study recognised by any of the Qualifying Bodies, shall not be reckoned as dating earlier than fifteen days before the date of Registration.

4. The Registration of Medical Students shall be placed under the charge of the Branch Registrars.

5. Each of the Branch Registrars shall keep a Register of Medical Students according to the subjoined Form:—

¹ The General Medical Council will not consider any Examination in English sufficient that does not fully test the ability of the candidate.—1st, To write a few sentences in correct English on a given theme, attention being paid to spelling and punctuation as well as to composition. 2d, To write a portion of an English author to dictation. 3d, To explain the grammatical construction of one or two sentences. 4th, To point out the grammatical errors in a sentence ungrammatically composed, and to explain their nature. 5th, To give the derivation and definition of a few English words in common use.

Form for the Registration of Medical Students.

Date of Registration.	NAME.	Preliminary Examination and Date.	Place of Medical Study.

6. Every person desirous of being registered as a Medical Student shall apply to the Branch Registrar of the division of the United Kingdom in which he is residing, according to the annexed Form, which may be had on application to the several Qualifying Bodies, Medical Schools, and Hospitals; and shall produce or forward to the Branch Registrar a Certificate of his having passed a Preliminary Examination, as required by the General Medical Council, and a statement of his place of Medical Study.

Form of Application for Registration as a Medical Student.

I hereby apply to be registered as a Student in Medicine, in conformity with the Regulations of the General Council of Medical Education and Registration of the United Kingdom, for which purpose I submit the following particulars:—

NAME OF APPLICANT. (To be written in words at length.)		Preliminary Examination.	Date of Preliminary Examination.	Place of Medical Study.
Surname.	Christian Name.			

Applicant's Signature ———.
Address ———.
Date of Application ———.

To the Registrar of the Branch Council for ———.

N.B.—The above Form of Application, duly and legibly filled up, must be forwarded to the Registrar, post free, and be accompanied by a Certificate of the Applicant's having passed a Preliminary Examination, as required by the General Medical Council; and a statement of his place of Medical Study.

The Certificate of Examination must testify that the Student has been examined in,—1. English Language, including Grammar and Composition; 2. Arithmetic, including Vulgar and Decimal Fractions; Algebra, including Simple Equations; 3. Geometry—First two books of Euclid; 4. Latin, including Translation and Grammar. And in one of the following optional subjects:—Greek; French; German; Natural Philosophy, including Mechanics, Hydrostatics, and Pneumatics.

7. The Branch Registrar shall enter the Applicant's name and other particulars in the Students' Register, and shall give him a Certificate of such Registration.

8. Each of the Branch Registrars shall supply to the several Qualifying Bodies, Medical Schools, and Hospitals, in that part of the United Kingdom

of which he is Registrar, a sufficient number of blank Forms of Application for the Registration of Medical Students.

9. The several Branch Councils shall have power to admit special exceptions to the foregoing regulations as to Registration, for reasons which shall appear to them satisfactory.

10. A copy of the Register of Medical Students, prepared by each of the Branch Registrars, shall be transmitted, on or before the 31st December in each year, to the Registrar of the General Council, who shall, as soon as possible thereafter, prepare and print, under the direction of the Executive Committee, an Alphabetical List of all Students registered in the preceding year, and supply copies of such authorized list to each of the Bodies enumerated in Schedule (A) to the Medical Acts, and through the Branch Registrars to the several Medical Schools and Hospitals.

11. The several Qualifying Bodies are recommended not to admit, after October 1870, to the final Examination for a Qualification under the Medical Acts, any Candidate (not exempted from Registration) whose name had not been entered in the Medical Students' Register at least four years previously.

In the case of Candidates from other than Schools of the United Kingdom, the Branch Councils shall have power to admit exceptions to this recommendation.

The Branch Councils are desired to take means to make these regulations known to the Medical Students at the various Medical Schools.

III.—AGE FOR LICENSE TO PRACTISE.

1. That the age of twenty-one be the earliest age at which a Candidate for any Professional License shall be admitted to his final Examination; that the age shall, in all instances, be duly certified; and that a Return of any exceptions to this recommendation allowed by the Licensing Bodies, together with the reasons for such exceptions, be transmitted to the Branch Council of that part of the United Kingdom in which they have been granted.

2. That no License be obtained at an earlier period than after the expiration of forty-eight months subsequent to the Registration of the Candidate as a Medical Student.

IV.—PROFESSIONAL EDUCATION.

1. That the course of Professional Study required for a License shall comprehend attendance during not less than four Winter Sessions, or three Winter and two Summer Sessions, at a School recognised by any of the Licensing Bodies mentioned in Schedule (A) to the Medical Act.

2. That the following are the subjects without a knowledge of which no Candidate should be allowed to obtain a qualification entitling him to be registered:—1. Anatomy. 2. General Anatomy. 3. Physiology. 4. Chemistry. 5. Materia Medica. 6. Practical Pharmacy. 7. Medicine. 8. Surgery. 9. Midwifery. 10. Forensic Medicine.

Chemistry should include a knowledge of the principles of Chemistry, and of those details of the science which bear on the study of Medicine.

Medicine and Surgery should include a knowledge of Systematic and Clinical Medicine and Surgery, and also of Morbid Anatomy.

3. That it be recommended to the several Licensing Bodies that the courses of instruction required by them be framed in such a manner as to secure a due share of attention, both to preparatory branches and to those more strictly connected with the Practice of Medicine and Surgery; and that it be suggested accordingly to these Bodies, that their regulations should be such as to prevent attendance upon Lectures from interfering with Hospital and Clinical Study.

4. That the Council will view with approbation any encouragement held out by the Licensing Bodies to Students to prosecute the study of the Natural Sciences before they engage in studies of a strictly professional character.

V.—PROFESSIONAL EXAMINATION.

1. That it is desirable that the different Licensing Bodies should combine their Examinations, when this is practicable, so as to secure that the knowledge of every Practitioner whose name appears on the Register shall have been tested in all the subjects of Professional Education which the Council has determined to be essential, viz.:—1. Anatomy. 2. General Anatomy. 3. Physiology. 4. Chemistry. 5. Materia Medica. 6. Practical Pharmacy. 7. Medicine. 8. Surgery. 9. Midwifery. 10. Forensic Medicine.

2. That those Licensing Bodies which have not already done so, be requested to furnish a statement of the dates of their Examinations and of the modes in which such Examinations are conducted, whether by written, oral, or practical Examination, and of the length of time a Candidate is under Examination in each or all of these ways; and that the Registrar transmit these Reports to the Members of the Council, in order that they may be taken into consideration at the next meeting of the several Branch Councils.

3. That the Professional Examination for any License be divided into two parts; the first embracing the primary or fundamental branches directly connected with the practice of Medicine and Surgery; that the former be not undergone till after the close of the Winter Session of the second year of professional study; and the latter, or final Examination, not till after the close of the prescribed period of professional study.

4. That the Examination in Physics, Botany, and Natural History may be undergone at an earlier period than the first Professional Examination.

5. That the Professional Examinations be conducted both in writing and orally; and that they be practical in all branches in which they admit of being so.

6. That not less than two Examiners, or one Examiner with an Assessor, should be present at every Oral Examination.

7. That the Oral Examinations should be so far public as to be open at least to the Medical and Surgical Graduates, or members of the Examining Body.

8. That the questions to be answered in writing should be so numerous, and embrace such a variety of the details of each subject, as may adequately test the proficiency of the Candidate; and that they should be submitted to the whole body of Examiners for consideration and revision, if desirable, before being proposed to the Candidates.

9. That the written answers should be submitted to more than one of the Examiners.

10. That excellence in one or more subjects should not be allowed to compensate for failure in others.

11. That if a Candidate be rejected for failure in any one subject, he should be re-examined in all.

12. That Examiners should only be elected for definite periods, with power of re-appointment.

13. That the Professional Examinations be held by the several Licensing Bodies, except in special cases, at stated periods, to be publicly notified.

14. That returns from the Licensing Bodies in Schedule (A) be made annually, on the 1st of January, and in the subjoined form, to the General Medical Council, stating the number and names of the Candidates who have passed their first as well as their second and third Examinations, and the number of those who have been rejected at the first and second and third Examinations respectively; and that the Registrar forward a sufficient number of forms, with a notice for their being returned in due time.

Table of Return of Professional Examinations, and their Results.

LICENSING BODIES.	QUALIFICATIONS.	NO. PASSED.			NO. REJECTED.		
		1st Exam.	2d Exam.	3d Exam.	1st Exam.	2d Exam.	3d Exam.

15. That for the future the Licensing Bodies, which do not already do so, distinguish separately in their Returns the names and numbers of Candidates examined, and the numbers of those passed and rejected at the examinations for each of the qualifications conferred.

16. That it is not desirable that any University of the United Kingdom should confer any degree in Medicine or Surgery, whether that of Bachelor, Doctor, or Master, upon Candidates who have not graduated in Arts, or passed all the Examinations required for the Bachelorship in Arts, or the Examinations equivalent to those required for a degree in Arts.

REMARKS ON MEDICAL STUDY.

NOTWITHSTANDING that Ichabod has been so long scrawled over the portal of our Edinburgh Medical School by the sacrilegious hands of those who would it were so, we still flourish more and more as we grow in years and honours. And so it comes to pass that once more we have to welcome to our Lecture-rooms and Infirmaries that mass of protoplasm which it is at once the pleasure and the duty of our Medical Teachers to aid in differentiating into the Medical Practitioners of the future. We supply the necessary nutriment, but they—the protoplasm—must provide the vital force necessary for assimilation and growth; a failure on either side would necessarily result in some one or other of those necro-biotic changes which would sweep us from our place with all the certainty and rapidity our worst foes could desire. But so long as both parties act honestly and with an earnest desire to do their best, there is no fear of the desired differentiation taking place after a normal and healthy fashion—even the coldness of our northern clime and nature is not found to be so excessive as to retard development, but merely sufficient to make it more sound and stable.

It is a matter of some importance to the Medical Student, after passing his preliminary examination and getting himself fairly registered as such, that he should make up his mind what degree or degrees or licenses he wishes to procure, and thereafter apply to the licensing bodies for their special regulations by which he may conduct his future studies. Every licensing body differs more or less in some one or more points, which look trivial on paper, and which possibly are of no great importance in reality, but which, when discovered only

at the last moment, may cause a delay of a whole session or more in the procuring of the coveted license—a delay which might very readily be prevented by a little timely forethought. In our pages we give abstracts of the regulations and requirements of the principal qualifying bodies, but these must only be looked upon as general information, and in every case the special regulations for the desired license should be procured, carefully studied, and the professional education conducted accordingly. The due attention to this will often save both time and money.

Having once fixed upon the course of study, we need not say that it should be followed fully and exactly. Most bodies require attendance upon lectures to be ascertained by calling the roll so many times each session, and certifying accordingly; and most students consider themselves diligent if they secure a full certificate, although they may have been often absent on other days, and when present have given only such a wandering attention as barely sufficed to keep them awake. But while much may be learned from a good lecturer which is not to be found in any book, much may also be learned from even the worst one, were it nothing more than that obtained from the inquiry which his doubtful statements are sure to elicit; and in every case it is of importance to keep up the continuous thread of the discourse, lest the student may have in his ladder of learning here and there a series of gaps, through one or other of which he may ignominiously fall just when he thinks himself most secure of gaining the summit of his ambition. For the Student may be sure that mere reading will hardly ever compensate for the want of oral instruction on any subject; and, even if it do so, will necessitate a much more strenuous application of the mental powers. To save trouble, therefore, as well as time, it is of the greatest importance to be diligent in attendance upon lectures, reading carefully over the subject in some good text-book at the same time, and making a special study and note of any apparent discrepancy between the lecture and the work. In this way a very competent knowledge of any subject is acquired with a readiness and ease quite unknown to those who, thinking to take it easy, blunder at the last into a quagmire of undigested knowledge, from which they may possibly escape by great personal exertion, and with the guidance of some well-skilled grinder. The student must always endeavour to make his knowledge real; many statements he must at first take upon trust, but he must accept them with reserve, and take the earliest possible opportunity of ascertaining their truth; and the teacher will also aid this spirit of inquiry by giving his reasons for every statement, or by pointing out why no certainty can be attained, and in what direction probability lies. This must always be especially the case in regard to Therapeutics, where experience plays so important a part, the reasons for this lying in the variable basis of living action with which we have to deal, in the fact that this action may be similarly affected in various ways, and in the certainty that most, if not all, curable diseases may get well of themselves. In the diagnosis of disease the matter is somewhat different; here experience plays a very subordinate, though often by no means an unimportant part; here the Student must be taught the habit of diligent observation; he must carefully collect all his facts; in regard to physical signs, he must pay careful heed to the physical conditions necessary for their development; then he must duly consider the general state and the various symptoms presented by the patient, and from these bases reason out the most probable cause for the physical facts discovered. And in regard to the examination of patients, it

must never be forgotten that before all it is needful to examine healthy subjects ; that as we can never know what is morbid in the structure of a lung unless thoroughly acquainted with the appearance of the healthy tissue, so we can never know what is morbid in the physical signs elicited, unless we perfectly understand what those physical signs are in health.

Under lately-passed Acts of Parliament Sanitary Boards and Sanitary Medical Officers are being appointed all over the kingdom ; the science of hygiene, the influence of the environment in the maintenance of health and production of disease, must therefore now take even a more important position than it has hitherto occupied ; and while the science of Therapeutics loses none of its importance or interest, the science of the Prevention of Disease is now assuming its rightful place. This is a matter which Medical Men have never neglected, and have often taken great pains to promote to their own detriment ; but the Student may be expected to pay more serious attention to this very important branch, now that emolument and not loss is connected with its exercise ; and, indeed, he must do so, if he desires to fit himself for any of those important positions which are now open to his attainment—positions in which he may gratify his ambition by practising philanthropy.

On the best method of pursuing Medical Studies, our counsel may be condensed in a few words. Four years are short enough for acquiring the extended information at present required by the Licensing Boards, even as a *minimum*. The Student therefore cannot too soon begin in earnest. During the session, he should lay out a plan of his hours of lectures, practical work, and private study, with sufficient intervals of healthful recreation ; and should carry out this plan with unswerving steadiness, ever remembering that steady work, and a habit of looking at all sides of a question or fact, is worth any amount of so-called talent and desultory exertion. During vacation, time may be properly spent in practical work, as assistant to a medical practitioner, or in reading, either to complete past work or in anticipation of the subjects of study for next session. To Students who wish to attain to high honours or position in the profession, we should certainly recommend the taking a Degree in Arts previous to commencing their professional studies ; while to those of more moderate views we should recommend five years, instead of four, for professional study ; or, at all events, they would find great advantage from an acquaintance with Natural History, Natural Philosophy, Chemistry, and Botany, before commencing their strictly Medical studies. This object might be attained by attending classes on these subjects during the summer, previous to the winter session which begins their Medical curriculum.

We would strongly advise all Students to join, after their first year, some of the debating societies which are now found in connexion with most medical schools, and of which the Royal Medical Society of Edinburgh, with its spacious rooms and large library, is an excellent example. The mental vigour derived from independent thought and free discussion, and the genial influences of friendship for which these societies afford opportunity, are of great value in the education of the Medical Student, and contribute, in no small degree, to a successful career in after life.

And we would also recommend all, after passing, to obtain an hospital appointment, or at least an assistantship in private practice—and it is perhaps wiser to take both—so that they may acquire the necessary confidence in themselves

and that experience in the ways of the world which is necessary to ensure success before they are thrown on their own resources.

The following pages contain a tabular abstract of the regulations of the various Licensing Boards, as well as a list of the Hospitals, Dispensaries, etc., attached to our Scotch Medical Schools; also the regulations for the Army, Indian, and Navy Medical Services. The space at our disposal does not allow of more detailed information. But, in point of fact, the regulations of all Licensing Boards now correspond much more closely than they used to do, and the regulations of the General Medical Council afford a key to the general requirements of all of them. For special information, application should always be made to the Secretaries of the Licensing Boards; or, in the case of the Universities, recourse may be had to the published Calendars.

The *Preliminary Examinations* are usually held before the commencement, and at the end, of the Winter Session,—viz., in October, March, or April—sometimes also at the end of the Summer Session. The Professional Examinations in Universities are usually after the Winter and during the Summer Sessions; in Edinburgh, however, the first Professional is held in October as well as April. The other Licensing Boards' examinations are held at various periods throughout the year. Special examinations, under circumstances of urgency, can be held at almost any time; but they, of course, entail considerable addition to the expense.

MEDICAL SCHOOLS OF SCOTLAND, 1873-74.

WINTER SESSION.

SUBJECTS.	UNIVERSITY OF EDINBURGH.	SCHOOL OF MEDICINE, EDINBURGH.	UNIVERSITY OF GLASGOW.	ANDERSON'S UNIVERSITY, GLASGOW.	UNIVERSITY OF ABERDEEN.	UNIVERSITY OF ST ANDREWS.
Anatomy, Systematic and Practical, with Demonstrations.	Professor Turner.	Dr Handyside.	Professor A. Thomson.	Dr George Buchanan.	Professor Struthers.	...
Physiology, or Institutes of Medicine.*	Professor Bennett.	Dr Bell Pettigrew.	Professor A. Buchanan.	Dr E. Watson.	Professor Ogilvie.	Professor Oswald Bell.
Chemistry, and Practical Chemistry.	Professor C. Brown.	Dr Macadam.	Professor T. Anderson.	Dr Thorpe.	Professor Brazier.	Professor Heddle.
Materia Medica and Therapeutics.	Professor Christison.	In Summer.	Professor Cowan.	Dr Morton.	In Summer.	...
Practice of Medicine.	Professor Laycock.	Dr Haldane,† Dr Muirhead. Dr Grainger Stewart.	Professor Gairdner.	Dr M'Call Anderson.	Professor Macrobain.	...
Surgery.	Professor Spence.	Dr Watson,† Dr Jos. Bell. Dr Chiene. Dr John Duncan.	Professor Macleod,‡	Dr Dunlop.	Professor Pirrie.	...
Midwifery.	Professor Simpson.	Dr J.M.Duncan.	Professor Leishman.	In Summer.	Professor Inglis.	...
Natural Philosophy.	Professor Tait.	Mr Lees.	Professor Sir Wm. Thomson.	Mr Forbes.	Professor Thomson.	Professor Swan.
Natural History.	Professor Wyville Thomson.	...	Professor Young.	...	Professor Nicol.	Professor Macdonald.
General Pathology.	Professor Sanders.	Dr Wyllie.	...	...	...	...
Clinical Medicine.	Professors Bennett, Laycock, MacLagan, and Sanders.	Drs Haldane,† Balfour, G. Stewart, and J.M.Duncan.	The Physicians of the Royal Infirmary.		Dr Smith, Beveridge, and Fraser.	..
Clinical Surgery.	Professor Lister.	Dr Watson,† Mr Annandale.	The Surgeons of the Royal Infirmary.		Drs Pirrie, Kerr, and Fiddes.	...

* This course is equivalent to that given under the name of General Anatomy and Physiology in the English Schools. Special schedules are issued by the London Boards for their Scotch students, which should always be inquired for.

† These are not conjoint courses, but separate ones by the gentlemen named.

‡ This is a joint course.

§ Dr Cameron also lectures extra-murally on Systematic Surgery, at 151 George Street, between 11 and 12.

MEDICAL SCHOOLS OF SCOTLAND, 1874.

SUMMER SESSION.

SUBJECTS.	UNIVERSITY OF EDINBURGH.	SCHOOL OF MEDICINE, EDINBURGH.	UNIVERSITY OF GLASGOW.	ANDERSON'S UNIVERSITY, GLASGOW.	UNIVERSITY OF ABERDEEN.
Practical Anatomy and Demonstrations.	Professor Turner.	Dr Handyside.	Professor A. Thomson.	Dr George Buchanan.	Professor Struthers.
Botany.	Professor Balfour.	...	Professor A. Dickson.	Mr Hennedy.	Professor Dickie.
Materia Medica.	...	Dr Fraser.	...	...	Professor Harvey.
Midwifery.	...	Drs Kellier * and Macdonald.	...	Dr Wilson.	...
Medical Jurisprudence.	Professor MacLagan.†	Dr Littlejohn.†	Professor Simpson.†	...	Professor Ogston.†
Comparative Anatomy.	Professor Turner.	Dr Handyside.	...	...	Prof. Nicol.
Histology.	Professors Bennett * and Sanders.	Drs Wyllie and Gamgee.	...	...	Professor Struthers.
Practical Chemistry.	Professor Crum Brown.	Dr Macadam.	Professor T. Anderson.	Dr Thorpe.	Prof. Brasier.
Natural Philosophy.	...	Mr Lees.	...	...	...
Natural History.	Professor Wyville Thomson.	...	Professor Young.	...	Prof. Nicol.
Clinical Medicine.	Professors Bennett, Laycock, MacLagan, and Sanders.	Drs Haldane,† Balfour, G. Stewart, and J. M. Duncan.	The Physicians of the Royal Infirmary.		Drs Harvey, Smith, and Beveridge.
Clinical Surgery.	Prof. Lister.	Dr Watson.* Mr Annandale.	The Surgeons of the Royal Infirmary.		Drs Pirrie, Kerr, and Fiddes.

Operative Surgery is taught during the Summer by Drs Chiene, Bell, and Duncan. Instruction in Vaccination is also given at the Royal Public Dispensary, Edinburgh, on Wednesdays and Saturdays at 12, both Summer and Winter, by Dr Husband; at the Faculty Hall, Glasgow, on Mondays at 12, by Dr Dunlop; and at the Royal Infirmary, Glasgow, on Mondays and Thursdays at 12 o'clock, by Dr Tannahill. Medical Psychology and Insanity are taught in Summer by Dr Laycock and Dr J. B. Tuke. The Diseases of Children by Dr Stephenson, and the Diseases of the Eye by Dr Argyll Robertson.

* These are not conjoint courses, but separate ones by the gentlemen named.

† Drs Simpson and Ogston deliver their courses only in Winter. Dr Littlejohn gives courses during both the Winter and Summer Sessions. Dr MacLagan also lectures during both Summer and Winter. The Winter Course is chiefly intended for law students, but is open to medical students also.

‡ This is a joint course.

*. For additional Summer Courses on special subjects, see the Prospectus of each School.

LIST OF HOSPITALS, DISPENSARIES, ETC., IN CONNEXION
WITH THE MEDICAL SCHOOLS OF SCOTLAND.

EDINBURGH.

ROYAL INFIRMARY, including **LOCK HOSPITAL**. Upwards of 560 Beds. Visits daily from 12 till 2 P.M. Physicians—Drs Bennett, Laycock, Mac-lagan, and Sanders, Professors of Clinical Medicine; Professor Simpson (for Diseases of Women); Drs D. R. Haldane, G. W. Balfour, and Granger Stewart, Clinical Lecturers; Dr J. Matthews Duncan (on Diseases of Women). Assistant Physicians—Drs Claud Muirhead and T. R. Fraser. Pathologist, Dr Pettigrew.

Surgeons—Mr Lister, Professor of Clinical Surgery; Professor Spence; Dr P. H. Watson; Mr Annandale; and Dr Joseph Bell. Assistant Surgeons, Drs J. Duncan and J. Chiene. Consulting Surgeons, Dr Dunsmure and Dr Gillespie. Ophthalmic Surgeons, Mr Walker and Dr Argyll Robertson. Dental Surgeon, Dr John Smith.

CONVALESCENT HOUSE, Corstorphine. Visiting Medical Officer, Dr Chiene.

CHALMERS HOSPITAL FOR THE SICK AND HURT. 24 Beds for medical and surgical patients. Physician, Dr Halliday Douglas. Surgeon, Dr P. H. Watson.

ROYAL MATERNITY HOSPITAL. 36 Beds; 245 in-patients and 336 out-patients annually. Consulting Physicians, Drs Moir and Graham Weir. Physicians, Drs Keiller, C. Bell, Ziegler, Matthews Duncan, and Professor Simpson. Consulting Surgeon, Dr Dunsmure.

ROYAL HOSPITAL FOR SICK CHILDREN. 60 Beds; average number of out-patients, about 5600. Consulting Physicians, Professor Sir Robert Christison, Dr C. Wilson, Dr Graham Weir, and Dr Matthews Duncan. Consulting Surgeon, Professor Spence. Physicians, Drs Stephenson, R. Peel Ritchie, J. Linton, and Brakenridge. Extra Physicians, Drs J. Dunsmure, jun., and J. Andrew. Surgeon-Dentist, Dr Smith. Pathologist, Professor Sanders. Ophthalmic Surgeon, Dr Argyll Robertson.

ROYAL PUBLIC DISPENSARY AND VACCINE INSTITUTION. About 12,700 patients annually. Medical Officers, Drs Ritchie, Stephenson, Cairns, Spence, Linton, G. W. Balfour, Sanders, Husband, D. Wilson, Smart, Andrew, and Moinet. Physician-Accoucheurs, Drs Keiller, Matthews Duncan, Stephenson, and Cairns. Superintendent of Vaccination, Dr Husband. Medical Secretary, Dr Andrew. Clinique daily at 2 P.M. Vaccination on Wednesdays and Saturdays at 12 noon. Apothecary, Mr R. Urquhart.

NEW TOWN DISPENSARY. About 9000 patients annually. Medical Officers, Drs Dycer, Joseph Bell, A. Dickson, Chiene, Cunynghame, Brakenridge, Affleck, Cadell, Dunsmure, Wyllie, Underhill, and Cumming. Physician-Accoucheurs, Drs Dumbreck, Weir, and Angus Macdonald. Superintendent of Vaccination, Dr Affleck. Medical Secretary, Dr Joseph Bell. Clinique daily at 2 P.M. Vaccination on Tuesdays and Fridays from 12 to 1.

ROYAL ASYLUM FOR THE INSANE. About 660 patients. Physician, Dr Clouston.

EYE INFIRMARY, 6 Cambridge Street, Lothian Road. Surgeons, Benjamin Bell, Esq., F.R.C.S., and Dr Joseph Bell, F.R.C.S. Open daily at 1 P.M. Average number of patients annually, 900.

EYE DISPENSARY, 54 Cockburn Street. About 2000 patients annually. Surgeons, Mr Walker, Dr Wilson, and Dr Argyll Robertson. Open Mondays, Wednesdays, and Fridays, at 1 P.M. Clinical instruction during the summer session.

EAR DISPENSARY, 54 Cockburn Street. Drs T. Keith and Jackson. Tuesdays at 12. Average, about 20 cases daily.

DENTAL DISPENSARY, 38 Cockburn Street. Consulting Surgeon, Professor Spence. Consulting Physician, Dr Sanders. Dental Surgeons, Dr Orphoot, Mr Hutchins, Dr Roberts, Dr Hogue, Mr Knox Chisholm, Mr Swanson. Assistant Dental Surgeon, Mr Chisholm, jun. Daily, 9 to 10 A.M. Average number of patients, 4000 per annum.

GLASGOW.

ROYAL INFIRMARY. 583 Beds. Visits daily at 9 A.M. Physicians, Drs W. T. Gairdner, Perry, M'Laren, M'Call Anderson, and Scott Orr. Physicians to out-patients, Drs Lindsay, Charteris, and Wood Smith.

Surgeons, Drs E. Watson, Dewar, G. H. B. Macleod, George Buchanan, and Morton. To out-patients, Drs Dunlop, Patterson, R. Watson, Smart, and Cameron. Pathologist, Dr Joseph Coats. Superintendent, Dr M. Thomas.

LOCK HOSPITAL. 81 Beds. Medical Officers, Drs James Dunlop and H. C. Cameron. In-patients, 520.

LYING-IN HOSPITAL AND DISPENSARY. 24 Beds; in-patients, 360; out-patients, 700. Physician-Accoucheurs, Drs J. G. Wilson and R. D. Tannahill. Assistant-Physicians, Drs R. T. Paton, G. Miller, H. Miller, and M. Hunter.

UNIVERSITY LYING-IN HOSPITAL, 152 Wellington Street. Medical Officers, Professor Leishman, Physician *ex officio*. Physicians, Drs John Wilson, Gavin P. Tennant, Murdoch Cameron. Dispensary for the Diseases of Women, on Mondays, Wednesdays, and Fridays, at 12 o'clock noon.

ROYAL ASYLUM FOR THE INSANE. About 680 patients. Physician-Superintendent, Dr A. Macintosh. Open to Students on payment of a small fee.

EYE INFIRMARY. 24 Beds; 132 in-patients; 3139 out-patients annually. Physician, Harry Rainy, M.D. Consulting Surgeon, Dr G. Buchanan. Surgeon, Thomas Reid, M.D. Assistant Surgeons, Matthew Charteris, M.D., R. Watson, M.D., H. C. Cameron, M.D., T. S. Meighan, M.B.

EAR DISPENSARY. About 400 patients annually. Surgeon, Dr Dewar. Open on Tuesdays, Thursdays, and Saturdays, at 3 P.M.

DISPENSARY FOR SKIN DISEASES. About 1400 patients. Physician, Dr M'Call Anderson. Practical Courses in Diseases of the Skin and Ear, during May, June, and July. The Dispensary is open on Mondays and Wednesdays at 4 P.M., and on Tuesdays for Diseases of the Ear at the same hour.

OPHTHALMIC INSTITUTION. Consulting Physician, Dr Charles Ritchie. Surgeon, Professor Macleod. Acting Surgeon, Dr J. R. Wolfe. Assistant Surgeon, Dr R. Bell. Open daily from 1 to 3 P.M. A Course of Lectures and Clinical Instruction during the winter and summer months.

GLASGOW DISPENSARY FOR DISEASES OF THE EAR, 241 Buchanan Street. During the Summer Session a Course of Lectures on Aural Surgery, and Practical Instruction in the Treatment of Ear Disease, is given to the Students attending the Clinique. Average number of cases treated, 800 annually. Surgeon and Lecturer, James Patterson Cassells, M.D., M.R.C.S. Lond.

The **HUNTERIAN MUSEUM** is open to Students of Medicine, for purposes of study, on purchase of the printed catalogue.

ABERDEEN.

ROYAL INFIRMARY. Upwards of 300 Beds. Visits daily at 12 o'clock. Physicians, Drs Harvey, Smith, and Beveridge.

Surgeons, Drs Kerr, Pirrie, Fiddes, and A. Ogston. Ophthalmic Surgeon, Dr Davidson. Pathologist, Dr Rodger. Dental Surgeon, Mr Williamson.

GENERAL DISPENSARY, LYING-IN AND VACCINE INSTITUTION. 7000 patients annually. Medical Officers, Drs Angus Fraser, Duncan, and Brown; Messrs

Smith, Paterson, and Fraser. Physician-Accoucheur, Dr Inglis. Dental Surgeon, Mr De Lessert. Visits daily at 9.30 A.M. Vaccination every Wednesday at 3 P.M.

LUNATIC ASYLUM. Above 300 patients. Consulting Physician, Dr Macrobain. Resident Physician, Dr Robert Jamieson.

EYE INSTITUTION, General Dispensary Buildings, Guestrow. Surgeon, Dr Davidson. Open daily, except Sunday, at 2.30 P.M. Clinical Instruction on Diseases of the Eye, and the use of the Ophthalmoscope. Average, 600 patients annually.

ARMY MEDICAL DEPARTMENT.

6 WHITEHALL YARD, S.W.

QUALIFICATIONS AND EXAMINATION OF CANDIDATES FOR COMMISSIONS IN THE ARMY MEDICAL SERVICE.

1. Every candidate desirous of presenting himself for admission to the Army Medical Service must be unmarried, and not under 21 nor over 28 years of age. He must produce a Certificate from the District Registrar, in which the date of birth is stated; or, if this cannot be obtained, an affidavit from one of the parents or other near relative, who can attest the date of birth, will be accepted. He must also produce certificates of moral character, one of them from the parochial minister, if possible.

2. The candidate must make a declaration that he labours under no mental or constitutional disease, nor any imperfection or disability that can interfere with the most efficient discharge of the duties of a medical officer in any climate.¹ He must also attest his readiness to engage for general service, and to proceed on foreign service when required to do so.

3. The candidate must be registered under the Medical Act of 1858 as licensed to practise Medicine and Surgery in Great Britain or Ireland.

4. Certificates of registration, character, and age must be lodged at the Army Medical Department at least one week before the candidate appears for examination.

5. Candidates will be examined by the Examining Board in the following subjects:—Anatomy and Physiology; Surgery; Medicine, including Therapeutics, the Diseases of Women and Children, Chemistry and Pharmacy, and a practical knowledge of drugs. (The examination in Medicine and Surgery will be in part practical, and will include operations on the dead body, the application of surgical apparatus, and the examination of Medical and Surgical patients at the bedside.) The eligibility of each candidate for the Army Medical Service will be determined by the result of the examinations in these subjects only. Candidates who desire it will be examined in French, German, and the Natural Sciences. A knowledge of Modern Languages being considered of great importance, intending competitors are urged to qualify in French and German. The Natural Sciences will include Comparative Anatomy, Zoology, Natural Philosophy, Physical Geography, and Botany, with special reference to *Materia Medica*, and the number of marks gained in these subjects will be added to the total number of marks obtained in the obligatory part of the Examination by candidates who shall have been found qualified for admission, and whose position on the list of successful competitors will thus be improved in proportion to their knowledge of these branches of science.

¹ His physical fitness will be determined by a Board of Medical Officers, who are required to certify that the candidate's vision is sufficiently good to enable him to perform any surgical operation without the aid of glasses. A moderate degree of myopia would not be considered a disqualification, provided it did not necessitate the use of glasses during the performance of operations, and that no organic disease of the eyes existed. Every candidate must also be free from organic disease of other organs, and from constitutional weakness or other disability likely to unfit him for military service in any climate.

6. After passing this examination, every candidate will be required to attend one entire course of practical instruction at the Army Medical School on—(1) Hygiene; (2) Clinical and Military Medicine; (3) Clinical and Military Surgery; (4) Pathology of Diseases and Injuries incident to Military Service.

7. At its conclusion, the candidate will be required to pass an examination on the subjects taught in the School. If he give satisfactory evidence of being qualified for the practical duties of an Army Medical Officer, he will be eligible for a commission as assistant-surgeon.

8. During the period of his residence at the Army Medical School each candidate will receive an allowance of 5s. per diem with quarters, or 7s. per diem without quarters, to cover all costs of maintenance; and he will be required to provide himself with uniform (*viz.*, the regulation undress uniform of an assistant-surgeon, but without the sword).

9. All candidates will be required to conform to such rules of discipline as the Senate may from time to time enact.

The examinations for admission to the service are held at the London University, Burlington Gardens, on the second Monday in February and August respectively, and on the following days.

The daily rates of pay of the officers of the Medical branch of the Hospital Department of our Army are as follows:—

Medical Staff.—Daily pay: Director-General, special; Surgeon-General £2, after twenty-five years' service £2, 5s., after thirty years' service £2, 7s., after thirty-five years' service £2, 10s.; Deputy Surgeon-General £1, 10s., after twenty-five years' service £1, 12s., after thirty years' service £1, 15s., after thirty-five years' service £1, 17s.; Surgeon-Major 17s. 6d., after fifteen years' service £1, after twenty years' service £1, 4s., after twenty-five years' service £1, 7s.; Surgeon, on appointment 10s., after five years' service 12s. 6d., after ten years' service 15s. Charge pay: the officer in medical charge of an army in the field, of 10,000 men and upwards, £1 daily; of 5000 men and upwards, 15s. daily; of less than 5000, 10s. daily. Or, in medical charge of a colony where the number of commissioned officers and enlisted men is 1500 and upwards, 5s. daily.

A Surgeon must pass such examination as the Secretary of State may require, before he can be promoted to the rank of Surgeon-Major.

Medical officers shall have the right to retire on half-pay after 20 years' service; medical officers of the rank of Surgeon-Major or Surgeon, shall be placed on the retired list at the age of 55, and all Surgeons-General and Deputy Surgeons-General at the age of 65 years.

Non-effective Pay.—A medical officer placed on half-pay by reduction of establishment, or on the report of a medical board in consequence of wounds or ill-health caused in and by the discharge of his duties, or on account of age, shall be entitled to half-pay at the following daily rates:—Surgeon-General, after thirty years' service, £1, 17s. 6d.; twenty-five years, £1, 13s. 6d.; twenty years, £1, 10s. Deputy Surgeon-General, after thirty years, £1, 5s. 6d.; twenty-five years, £1, 2s. 6d.; twenty years, £1, 1s. Surgeon-Major, after fifteen years, £1; twenty years, 16s. 6d. Surgeon, after fifteen years, 13s. 6d.; ten years, 10s.; after five years, 8s.; under five years, 6s.

The rate of half-pay awarded to officers retiring for their own convenience, after twenty years' service on full-pay, shall not exceed one-half of their full-pay at the time of retirement.

ARMY MEDICAL SCHOOL.

ROYAL VICTORIA HOSPITAL, NETLEY.

President of the Senate.—Sir T. Galbraith Logan, K.C.B., M.D., Director-General of the Army Medical Department.

Members of the Senate.—Sir Ranald Martin, C.B., Physician to the Indian Council; Surgeon-General T. G. Balfour, M.D., Principal Medical Officer,

Royal Victoria Hospital; Surgeon-General T. Longmere, C.B., Professor of Military Surgery; Surgeon-General W. C. Maclean, Professor of Military Medicine; William Aitken, M.D., Professor of Pathology; E. A. Parkes, M.D., F.R.S., Professor of Military Hygiene.

Assistant Professors.—Staff-Surgeon-Majors T. H. Porter, W. H. Webb, and F. S. B. F. De Chaumont, M.D., and Surgeon F. H. Welch.

Candidates for Commissions in the Army, and in the Queen's Indian Service, proceed to Netley after passing the Examination at London. At Netley they attend the Medical and Surgical Practice of the Royal Victoria Hospital, and learn the system and arrangements of Military Hospitals. During four months they attend the lectures given by the Professors and Assistant-Professors, and go through a course of practical instruction in the Hygienic Laboratory and Microscopical Room. The lectures and practical instruction are intended to explain the specialities of Military Medical Practice, attention being directed to gunshot and other wounds, surgical arrangements in the field during action and sieges, means of transport, field hospitals, tropical diseases and their means of investigation, service in India and in the various colonies, the sanitary arrangements in peace and war, and the means of carrying out the sanitary regulations. Every opportunity is taken of practising operations on the dead body, and practical points of a like kind.

INDIAN MEDICAL SERVICE.

INDIA OFFICE, WHITEHALL, S.W.

ALL natural-born subjects of Her Majesty between twenty-two and twenty-eight years of age at the date of the examination, and of sound bodily health, may be candidates for the appointment of assistant-surgeon in Her Majesty's Indian Medical Service.

They must subscribe and send in to the Military Secretary, India Office, Westminster, proofs of their qualification similar to those required by the Army Medical Department.

Degrees, diplomas, licenses, and certificates of their registration in accordance with the Medical Act of 1858, must be lodged at the India Office, for examination and registry, at least one fortnight before the candidate appears for examination. The regulations, subjects, and times for examination are the same as for the Army Medical Department. Passage allowance to India on appointment will be given, or a passage provided. When passages are provided on board the Indian troop-ships, a charge for messing will be made at the rate laid down in the Royal Passage Warrant of 1865. Pay at 10s. a day will be allowed from the date of passing final examination at the Army Medical School.

Indian allowances and time of service for pension will reckon from date of arrival in India. The period of residence at the Army Medical School will reckon as service for the full-pay pension only.

The duties will be those hitherto performed by the medical officers of the East India Company's Service, with the exception of those relating to European troops.

Surgical instruments are provided in India by the Government for the use of medical officers.

When assistant-surgeons have served the requisite time they will be examined in the following manner as a test for promotion:—

A series of printed questions, prepared by the Inspector-General of the Presidency, will be sealed and sent to the principal medical officers of stations where assistant-surgeons may be eligible for examination. It will be the duty of the principal medical officer of the station to deliver these sealed questions to the assistant-surgeons, and to see that they are answered without the assistance of books, notes, or communication with any other person. The answers

are to be signed, and delivered sealed to the principal medical officer, who is to send them, unopened, to the Inspector-General of the Presidency, together with a certificate from the surgeon of the regiment, or other superior medical officer, that the assistant-surgeon has availed himself of every opportunity of practising surgical operations on the dead body.¹

The assistant-surgeon will also be required to transmit, together with his answers to the Inspector-General of the Presidency, a medico-topographical account of the station where he may happen to be at the time, or of some other station where he may have been resident sufficiently long to enable him to collect the necessary information for such a report. Failing this, he will send a medico-statistical report of his regiment for a period of at least twelve months.

If the Inspector-General of the Presidency is satisfied with the replies to the questions, and with the certificates and medico-topographical or statistical report, the assistant-surgeon will be held qualified for promotion.

The assistant-surgeon will thus be subjected to three separate examinations within the first ten years of his service, each examination having a definite object. The first, to ascertain, previous to his admission into the service as a candidate, his scientific and professional education, and to test his acquirements in the various branches of professional knowledge; the second, after having passed through a course of special instruction in the Army Medical School, to test his knowledge of the special duties of an army medical officer; and the third, previous to his promotion, to ascertain that he has kept pace with the progress of medical science.

The promotion of assistant-surgeons will be regulated by length of service, and not, as heretofore, by succession to vacancies in a fixed establishment of officers of the higher grades.

Assistant-surgeons of twelve years' service from the date of first commission (of which two years shall have been passed in charge of a native regiment), who shall have passed the prescribed examination in professional subjects, will be promoted to the rank of surgeon.

A surgeon, whether on the staff or attached to regiments, must have served ten years in India, of which two must have been passed, with the rank of surgeon, in or with a regiment, or as a civil surgeon, before he will be eligible for promotion to the rank of deputy inspector-general of hospitals.

A deputy inspector-general of hospitals must have served three years in India as such before he shall be eligible for promotion to the rank of inspector-general.

In cases, however, of emergency, or when the good of the service renders such alteration desirable, it shall be competent for the Governor-General in Council to shorten the several periods of service above mentioned, in such manner as he shall deem fit and expedient.

With a view to maintain the efficiency of the service, all medical officers below the rank of deputy inspector-general of hospitals shall be placed on the retired list when they shall have attained the age of fifty-five years, and all inspectors-general and deputy inspectors-general when they shall have attained the age of sixty-five years.

With a view of promoting the efficiency of the service, it has been further determined that the tenure of office by a deputy inspector-general shall, as in the case of inspectors-general, be limited to five years; officers being, however, if not disqualified by age, eligible either for employment for a second tour of duty in the same grade, or for employment in the higher grade of inspector-general by promotion thereto.

The rank of inspector-general and deputy inspector-general conferred upon officers of the Indian Medical Service under the royal warrant of 13th January 1860 is to be considered as substantive rank.

These officers, on vacating office at the expiration of the five years' tour of duty, will be permitted in future to draw respectively an unemployed salary of 1200 rupees per mensem in the former, and 900 rupees in the latter case, for a

¹ The assistant-surgeon may see this certificate before it is sent to the Inspector-General.

period of six months from the date of their vacating office, after which they will be placed while unemployed on the rate of pay laid down for officers of corresponding rank in Europe. These sums, deducted from the consolidated salary, will regulate the moiety of staff salary to be drawn by officers of those grades during absence on sick certificate.

NAVY MEDICAL DEPARTMENT.

Admiralty, 9 New Street, Spring Gardens, S.W.

REGULATIONS RELATIVE TO THE EXAMINATION OF CANDIDATES FOR THE APPOINTMENT OF ASSISTANT-SURGEON IN THE ROYAL NAVY.

1. That a candidate for entry into the Royal Navy shall make a written application to that effect, addressed to the Secretary of the Admiralty, on the receipt of which application he will be furnished with the regulations, and a printed form, to be filled up by him, to show if he possesses the required qualification.

2. As vacancies occur, the number of candidates required will be ordered to attend for competitive examination at the Admiralty Office; but it is to be observed, that no person can be admitted as an assistant-surgeon in the Royal Navy unless he can produce satisfactory evidence that his name has been placed on the Medical Register as legally qualified to practise both Medicine and Surgery according to the Regulations established by the Medical Act. And further, he will be required to make a declaration that he is free from any mental or bodily disease, defect, or infirmity, which could interfere with the efficient discharge of his duties as a medical officer in the Navy.¹

3. Each candidate will be required to produce a certificate of good moral character, signed by the clergyman of the parish, or by the magistrate of the district, or by a legally qualified physician or surgeon.

4. A certificate that he is not less than twenty nor more than twenty-eight years of age.

5. Candidates will be examined by the Examining Board in the following subjects:—Anatomy and Physiology; Surgery; Medicine, including Therapeutics, the Diseases of Women and Children; Chemistry and Pharmacy, and a practical knowledge of drugs. (The examination in Medicine and Surgery will be in part practical, and will include operations on the dead body, the application of surgical apparatus, and the examination of medical and surgical patients at the bedside.) The eligibility of each candidate for the Naval Medical Service will be determined by the result of the examinations in these subjects only.

Candidates who desire it will be examined in Comparative Anatomy, Zoology, Natural Philosophy, Physical Geography, and Botany, with special reference to *Materia Medica*; also in French and German; and the number of marks gained in these subjects will be added to the total number of marks obtained in the obligatory part of the examination by candidates who shall have been found qualified for admission, and whose position on the list of successful competitors will thus be improved in proportion to their knowledge of these branches of science.

6. After passing this examination, every candidate will be required to attend one entire course of practical instruction in the Medical School at Netley, on—(1) Hygiene; (2) Clinical and Naval and Military Medicine; (3) Clinical and Naval and Military Surgery; (4) Pathology of Diseases and Injuries incidental to Naval and Military Service.

7. At its conclusion, the candidate will be required to pass an examination

¹ His physical fitness will be determined by a Board of Medical Officers, who will be required to certify that his vision is sufficiently good to enable him to perform any surgical operation without the aid of glasses, and that he is free from organic disease, constitutional weakness, or other disability likely to unfit him for naval service in any climate.

on the subjects taught in the School. If he give satisfactory evidence of being qualified for the practical duties of a Naval Medical Officer, he will be eligible for a commission as Surgeon.

8. During the period of his residence at the Netley Medical School, each candidate will receive an allowance of 5s. per diem with quarters, or 7s. per diem without quarters, to cover all cost of maintenance; and he will be required to provide himself with uniform—viz., the regulation undress uniform of a Surgeon, but without the sword.

The full and half pay of Naval Medical Officers is in accordance with the following scale:—Full-pay: Inspector-General of Hospitals and Fleets—On promotion or under twenty-five years' service, £2, 5s.; ditto or above twenty-five years' service, £2, 6s.; and for each additional year of service 1s. a day more until the maximum is reached—namely, £2, 10s. Deputy Inspector-General of Hospitals and Fleets—On promotion or under twenty-two years' service, £1, 11s.; ditto or above twenty-two years' service, £1, 12s.; and for each additional year of service 1s. a day more until the maximum is reached—namely, £1, 18s. Staff Surgeon—On promotion or under twenty years' service, £1, 3s.; ditto or above twenty years' service, £1, 4s.; and for each additional year of service 1s. a day more until the maximum is reached—namely, £1, 10s. Staff Surgeon 2d Class—On promotion or under fourteen years' service, 18s.; ditto or under seventeen years' service, £1; and for each additional year of service 1s. a day more until the maximum is reached—namely £1, 2s. Surgeon—Under five years' service, 11s.; under eight years' service, 12s. 6d.; under eleven years' service, 14s.; under fourteen years' service, provided he passed his examination for Staff Surgeon 2d Class while under ten years' service, 15s. 6d.; above fourteen years' service, ditto, 17s. Half-pay: Surgeon—Under five years' service, 6s.; under eight years' service, 8s.; under eleven years' service, 10s.; above eleven years' service, provided he passed his examination for Surgeon while under ten years' service, 11s. Staff Surgeon 2d Class—On promotion or under fourteen years' service, 11s.; ditto or under seventeen years' service, 13s.; ditto or above seventeen years' service, 14s. Staff Surgeon—On promotion or under twenty years' service, 16s.; ditto or above twenty years' service, 16s. 6d.; and for each additional year of service 6d. a day more until the maximum is reached—namely, 18s. 6d. Deputy Inspector-General of Hospitals and Fleets—On promotion or under twenty-two years' service, £1, 1s.; ditto or above twenty-two years' service, £1, 2s.; and for each additional year of service 1s. a day more until the maximum is reached—namely, £1, 7s. Inspector-General of Hospitals and Fleets—On promotion or under twenty-five years' service, £1, 11s.; ditto or above twenty-five years' service, £1, 12s.; and for each additional year of service 1s. a day more until the maximum is reached—namely, £1, 18s.

PRELIMINARY EXAMINATIONS for the DIPLOMAS of the ROYAL COLLEGES of PHYSICIANS and SURGEONS of EDINBURGH, during the Year 1873-74.

1. The *Preliminary Examination in General Education* for the Double Qualification in Medicine and in Surgery conferred conjointly by the Royal Colleges of Physicians and Surgeons, and also for the separate Diploma of each college, for 1873-74, will embrace the following subjects:—

1. *English Language*, including Grammar and Composition.
2. *Arithmetic*, including Vulgar and Decimal Fractions.
3. *Algebra*, including Simple Equations.
4. *Geometry*; First Two Books of Euclid.
5. *Latin*; Translation from Latin into English, Parsing, and rendering English correctly into Latin, the Latin words being supplied.
6. One of the following subjects at the option of the candidate:—(1

Greek; (2.) *French*; (3.) *German*; (4.) *Natural Philosophy*, including *Mechanics*, *Hydrostatics*, and *Pneumatics*.

II. In *Latin*, the books prescribed to be translated from are either Cicero de Senectute et de Amicitia, or Horatii Carmina, Lib. II. et III.; and the candidate, besides selecting either of these two at his option, will be required to translate a passage from an unprescribed author.

In *Greek*, the books required are, Herodotus' History, Book I., and Homer's Iliad, Book II. On each occasion, translation from both the books prescribed is required, also parsing from the passages translated, and derivations of English words from the Greek.

In *French*, the book prescribed is Voltaire's *Henriade*. Parsing and translation of short sentences from English into French are also required.

In *German*, the book prescribed is Schiller's *Wilhelm Tell*. Parsing and translation of short sentences from English into German are also required.

III. The examinations will be held on the following days, beginning each day at twelve o'clock:—Tuesday, October 21st, and Wednesday, October 22d, 1873; Tuesday, April 21st, and Wednesday, April 22d, 1874; and Saturday, July 25th, and Monday, July 27th, 1874. On each occasion the subjects of the first day's examination will consist of English, Latin, and Geometry; and those of the second day, of Arithmetic, Algebra, and the optional subjects.

Candidates are required to give in their names to the officer of either College not less than two days prior to the day of examination.

Each candidate shall pay a fee of ten shillings previous to the examination. In the event of the candidate being unsuccessful, he shall be allowed to appear again for examination without paying a fee.

IV. The examinations will be conducted according to the following regulations:—

1. The competence of the candidate will be ascertained by means of written exercises. Candidates whose success amounts to a certain minimum will be admitted.

2. Each candidate must bring with him a card having his name, place of birth, and an address that will find him by post distinctly written on it.

3. The exercises on each of the subjects must, in every case, be written on separate papers; and the candidate must attach his signature and his address to each sheet of his written paper before giving it in to those who superintend the examination.

4. Books must not be employed, nor may assistance be given by one candidate to another, during the examination. Those who violate this necessary rule shall forfeit, for the season, the right of being examined.

5. The decision of the examiners will be intimated to each candidate as soon as possible. Those who pass the examination will receive certificates to that effect. In the case of those who are successful in part of the examination only, the subjects in which they have passed will be recorded, and they will not be again examined in the branch or branches in which they have been successful.

V. A certificate of having passed all the required subjects of the above examination, entitles the possessor, on commencing attendance on medical classes, to be registered as a Medical Student in the form required by the regulations of the General Medical Council, and, provided the candidate passes in three books of Euclid, is also admitted *pro tanto* by the Medical Faculty of the University of Edinburgh as an equivalent for the corresponding subjects of their matriculation examination.

A list of Examining Boards in the United Kingdom and in the Colonies, whose certificate is received by the Royal Colleges, on the recommendation of the General Medical Council, as equivalent to the above, will be found in the Appendix to the Regulations of the Colleges.

ROBERT PATERSON, *President Royal College of Physicians.*

WILLIAM WALKER, *President Royal College of Surgeons.*

EDINBURGH, 2d June 1873.

Frequent inquiries having been made as to the exact nature of the Preliminary Examination for the Diploma of the Royal Colleges of Physicians and Surgeons of Edinburgh, the Secretaries have supplied the following papers, which were given out at the examination of 30th of July last.

FIRST DAY.—COMPULSORY.

ENGLISH.

The candidate is required :—

1. To write a passage dictated by the examiner, with strict attention to spelling and punctuation.

2. To give the meaning and derivation of at least eight of the following words :—

Proprietor, Verification, Experience, Credibility, Sensation, Demonstration, Conviction, Universal, Moral, Study, Accuracy, Typical, Physician.

3. To parse the following lines :—

“ Gives not the hawthorn bush a sweeter shade
To shepherds looking on their silly sheep,
Than doth a rich embroider'd canopy
To kings, that fear their subjects' treachery ? ”

4. To write a short essay on “ The visit of the Shah of Persia to England.”
[The essay not to exceed a folio page.]

GEOMETRY.

Euclid, Books 1 and 2.

1. If from the ends of one side of a triangle, two straight lines be drawn to a point within the triangle, these two straight lines shall be less than the other two sides of the triangle, but shall contain a greater angle.

2. Triangles upon equal bases, and between the same parallels, are equal to one another.

3. If the square described upon one of the sides of a triangle be equal to the squares described upon the other two sides, the angle contained by these two sides is a right angle.

4. If a straight line be divided into two equal and also into two unequal parts, the rectangle contained by the unequal parts, together with the square of the line between the points of section, is equal to the square of half the line.

5. Also the squares of the two unequal parts are together double of the square of half the line, and of the square of the line between the points of section.

Euclid, Book 3.

1. If a straight line drawn through the centre of a circle bisect a straight line in the circle which does not pass through the centre, it will cut it at right angles; and if it cut it at right angles it will bisect it.

2. The diameter is the greatest straight line in a circle; and of all others that which is nearer to the centre is always greater than one more remote; and conversely.

3. If a straight line touch a circle, and from the point of contact a straight line be drawn cutting the circle, the angles made by this line with the line which touches the circle are equal to the angles in the alternate segments of the circle.

LATIN.

I.—*Horace.*

Translate :—

Eheu, fugaces, Postume, Postume,
Labuntur anni: nec pietas moram
Rugis et instanti senectae
Afferet indomitaequae morti.

Non si trecentis, quotquot eunt dies,
 Amice, places illacrymabilem
 Plutona tauris; qui ter amplum
 Geryonem Tityonque triasti
 Compescit unda, scilicet omnibus
 Quicumque terrae munere vescimur
 Enaviganda; sive reges,
 Sive inopes erimus coloni.
 Frustra cruento Marte carebimus,
 Fractisque rauci fluctibus Adriae:
 Frustra per Autumnos nocentem
 Corporibus metuemus Austrum.

Parse Labuntur. Conjugate *afferet*,—give the third person singular of the imperfect, perfect, pluperfect, present future indicative active, and of the present and imperfect subjunctive active. *Parse places*,—give English words derived from it. *Parse munere* and *vescimur*. *Parse corporibus*,—decline the singular and plural.

Numerical value, 50.

II.—Cicero.

Scipio. Saepenumero admirari soleo cum hoc C. Laelio, tum ceterarum rerum tuam excellentem, M. Cato, perfectamque sapientiam, tum vel maxime, quod numquam senectutem tibi gravem esse senserim: quae plerisque senibus sic odiosa est, ut onus se *Aetna* gravius dicant sustinere. *Cato.* Rem haud sane difficilem, Scipio et Laeli, admirari videmini; quibus enim nihil opis est in ipsis ad bene beateque vivendum, iis omnis gravis est aetas: qui autem omnia bona a se ipsis petunt, iis nihil potest malum videri, quod naturae necessitas afferat, quo in genere in primis est senectus, quam ut adipiscantur, omnes optant: eandem accusant adepti: tanta est inconstantia stultitiae, atque per-versitas. Obrepere, aiunt, eam citius, quam putassent.

Parse soleo,—conjugate it. Compare *maxime*. *Parse senserim*. *Parse Aetna gravius*. *Parse opis*. Compare *malum*. Is there any other Latin word *malum*? *Parse adipiscantur*. Compare *citius*. *Parse putassent*.

Numerical value, 50.

III.—From an unprescribed Author.

Nam, uti genus hominum compositum ex anima et corpore, ita res cunctae, studiaque omnia nostra, corporis alia, alia animi naturam sequuntur. Igitur praeclara facies, magnae divitiae, ad hoc vis corporis, alia hujusmodi omnia, brevi dilabuntur; at ingenii egregia facinora, sicuti anima, immortalia sunt.

What word is understood after *brevi*? *Parse facinora*.

Numerical value, 50.

IV.—Passage to be rendered in correct Latin.

The contest was dubious till his army broke into the town. In that battle, being wounded under the breast, he began to faint through loss of blood; yet he fought upon his knees, till he killed him by whom he had been wounded. The dressing of the wound was more painful than the wound itself.

Certamen sum anceps donec exercitus irrumpo in oppidum. In is praedium, trajectus sub mamma, coepi deficio fluens sanguis; tamen praelior genu, donec occido is a qui vulnere. Curatio vulnus sum gravis ipse vulnus.

Numerical value, 50.

In order to pass, the candidate must do one or other of Nos. I. and II.; he must also do Nos. III. and IV.; and have obtained 100 marks in all.

SECOND DAY.—COMPULSORY.

ARITHMETIC.

1. Add together $\frac{1}{2}$ of £174, 11s. 3d., $\frac{1}{3}$ of £107, 8s. 9d., and $\frac{1}{4}$ of £182, 13s. 6d.
2. Divide 15 tons, 3 cwts. 2 qrs. 11 lbs. 13 ozs. by $6\frac{1}{2}$.
3. If 7 men can reap a field in $18\frac{1}{2}$ hours, in how many hours can 15 men reap the same field?
4. Add together $\cdot 015$ of a £, $\cdot 025$ of a crown, and $\frac{1}{4}$ of $2\frac{1}{2}$ of a shilling?
5. Express as a single fraction in its lowest terms the sum of $\frac{1}{12} + \frac{1}{15} + \frac{1}{18} + \frac{1}{24} + \frac{1}{30}$.
6. Find the amount of £1044 in 3 years at 4 per cent. compound interest.
7. Find the alteration in income produced by shifting £1000 3 per cents. at 93 to a stock yielding 4 per cent. at 110, the brokerage in each case being £5.
8. What sum will amount to £1000 in 4 months, interest at the rate of $\frac{1}{4}$ per cent. per annum.

N.B.—The working of each question, as well as the answer, must be given in full.

ALGEBRA.

1. If $a = \frac{1}{2}$, $b = \frac{1}{3}$, $c = \frac{1}{4}$, $d = \frac{1}{5}$, find the value of $\frac{abc + abd + acd}{bcd}$ and of $\frac{a^2 - b^2}{a - b}$.
2. Multiply together $x^2 - x + 1$, $x^2 + x - 1$, and $x^2 + x + 1$.
3. Divide $a^2 - 6a + 5$ by $a^2 - 2a + 1$.
Write down the remainder from dividing $x^2 - 11x + 12$ by $x - 1$.
4. Express as single fractions in their lowest terms:—
 - (1.) $\frac{a^2 - b^2 - c^2 + 2bc}{a^2 + 2ab + b^2 - c^2}$.
 - (2.) $\frac{1}{2x + y} - \frac{1}{2x - y} + \frac{4x}{4x^2 - y^2}$.
5. Solve the following equations:—
 - (1.) $\frac{1}{4}(x - 9) + \frac{1}{3}(x - 5) = \frac{1}{2}(x - 7) + 1\frac{1}{2}$.
 - (2.) $\frac{x}{1 - 3x} - \frac{1 - 2x^2}{3x^2 - 1} = \frac{1}{2}$.

N.B.—The working of each question, as well as the answer, must be given in full.

SECOND DAY.—OPTIONAL.

GREEK.

I. (Homer, *Iliad*, Lib. II.)

Translate:—

Κλῦτε, φίλοι, θεῖός μοι ἐνύπνιον ἦλθεν Ὀνειρος,
 Ἀμβροσίην διὰ νύκτα· μάλιστα δὲ Νέστορι δίψ,
 Εἰδὸς τε, μέγεθος τε, φύνη τ', ἀγχιστα ἐψέκει.
 Στῆ δ' ἄρ' ὑπὲρ κεφαλῆς, καὶ με πρὸς μῦθον ἔειπεν·
 Εὔδεις, Ἀτρεΐος υἱέ, δαιφρονος, ἱπποδάμοιο;
 Οὐ χρὴ παννύχιον εὔδειν βουληφόρον ἄνδρα,
 ὅτι λαοὶ τ' ἐπιτετράφαται, καὶ τόσσα μέμηλε.
 Νῦν δ' ἐμέθεν ξύνει ὥκα· διὸς δέ τοι ἀγγελός εἰμι,
 Ὅς σευ, ἄνευθεν ἔων, μέγα κήδεαι, ἧδ' ἐλαίρει·
 Θωρήξαι σε κέλευσε καρηκομῶντας Ἀχαιοὺς
 Πανσυδῆ· νῦν γάρ κεν ἔλοις πόλιν εὐρύγων
 Τρώων· οὐ γὰρ ἐτ' ἀμφὶς Ὀλύμπια δώματ' ἔχοντες
 Ἀθάνατοι φράζονται·

Parse κλῦτε. Compare μάλιστα. Compare ἀγχιστα. Parse εὔδεις,—give the first person singular of its imperfect and of its future. Parse ἐπιτετράφαται. Parse ἐμέμηλε. Parse ξύνει. Parse ἔλοις.—Numerical value, 50.

II. (*Herodotus, Book I.*)

Τελευτήσαντος δὲ Ἀλυσίου, ἐξεδήξατο τὴν βασιλὴν Κροίσος ὁ Ἀλυσίου, ἐτέων ἑὼν ἡλικίην πέντε καὶ τριῶντα· οἱ δὲ Ἕλληνας πρῶτοις ἐπεθήκατο Ἐφέσιοι. ἔνθα δὲ οἱ Ἐφέσιοι πολιορκούμενοι ὑπ' αὐτοῦ ἀνέθεσαν τὴν πόλιν τῇ Ἀρτέμει, ἐξάψαντες ἐκ τοῦ νηοῦ σκουρίων ἐν τῷ τεύχεσι. ἔστι δὲ μεταξὺ τῆς τε παλαιῆς πόλεως, ἣ τότε ἐπολιορκέτο, καὶ τοῦ νηοῦ ἑπτὰ στάδια. πρῶτοις μὲν δὴ τούτοις ἐπεχείρησε ὁ Κροίσος· μετὰ δὲ ἐν μέρει ἐκάστοις Ἰώνων τε καὶ Αἰολέων, ἀλλοις ἄλλας αἰτίας ἐπιφέρων, τῶν μὲν ἰδίωτας μέζοντας παρενύσκειν, μέζοντα ἐπαιτῶμενος, τοῖσι δὲ αὐτέων καὶ φαῖλα ἐπιφέρειν.

Parse τελευτήσαντος, ἐξεδήξατο, ἐτέων, ἐπεθήκατο, πολιορκούμενοι,—give its derivation. Parse ἐξάψαντες, ἐπεχείρησε, μέρει. Compare μέζοντας.—Numerical value, 50.

III.

Give the derivation of the following words from the Greek:—Ephemeral, Metropolis, Glossary, Schism, Pantheism, Angel, Oligarchy, Democrat, Aristocrat, Heliotrope, Ichthyology, Monograph, Autograph, Holograph, Photograph, Telegraph, Biography, Geography, Exodus, Deuteronomy, Genesis, Economy.—Numerical value, 50.

[The Candidate to pass must have attained to 75 marks.]

FRENCH.

1. Translate into English:—

Mayenne, dont les yeux savent tout éclairer,
Voit le coup qu'on prépare, et feint de l'ignorer.
De ce crime odieux son prudent artifice
Songe à cueillir le fruit sans en être complice :
Il laisse avec adresse aux plus séditieux
Le soin d'encourager ce jeune furieux.

Tandis que des ligueurs une troupe homicide
Aux portes de Paris conduisait le perfide,
Des Seize en même temps le sacrilège effort
Sur cet événement interrogeait le sort.
Jadis de Médicis l'audace curieuse
Chercha de ces secrets la science odieuse,
Approfondit longtemps cet art surnaturel,
Si souvent chimérique, et toujours criminel.
Tout suivit son exemple; et le peuple imbécile,
Des vices de la cour imitateur servile,
Epris du merveilleux, amant des nouveautés,
S'abandonnait en foule à ces impiétés.

2. Give the first person singular of the leading tenses of the following verbs, with their past participle:—Feint, cueillir, suivit, épris, s'abandonnait.

3. Correct the errors in the following:—La reine prevoyante le danger. Ou est la plume? Il l'ai perdu.

4. Translate into French (giving the numerals in full):—Queen Victoria was born on the twenty-fourth of May eighteen hundred and nineteen. She ascended the throne on the death of her uncle William the Fourth. She is beloved by all her subjects.

GERMAN.

1. Translate into English:—

O eine edle Himmelsgabe ist
Das Licht des Auges! Alle Wesen leben
Vom Lichte, jedes glückliche Geschöpf—
Die Pflanze selbst kehrt freudig sich zum Lichte.
Und er muss sitzen, fühlend, in der Nacht,
Im ewig Finstern—ihn erquickt nicht mehr

Der Matten warmes Grün, der Blumen Schmelz,
Die rothen Firnen kann er nicht mehr schauen—
Sterben ist nichts—doch leben und nicht sehen,
Das ist ein Unglück.—Warum seht ihr mich
So jammerend an? Ich hab zwei frische Augen
Und kann dem blinden Vater keines geben,
Nicht einen Schimmer von dem Meer des Lichts,
Das glanzvoll, blendend mir ins Auge dringt.

Stauffacher.

Ach, ich muss euren Jammer noch vergrößern,
Statt ihn zu heilen—Er bedarf noch mehr!
Denn alles hat der Landvoght ihm geraubt;
Nichts hat er ihm gelassen als den Stab,
Um nackt und blind von Thür zu Thür zu wandern.

2. Parse the following verbs,—*Kann, Schauen, Sehen, Geben, Dringt, Bedarf*,—giving the leading tenses.

3. Translate into German:—

Ausdorf.—Good morning children! Well, how are you?

Alfred.—You are heartily welcome.

Ausdorf.—My children, I have not seen you for a long time; but perhaps you think of us sometimes.

Emma.—Always, father, always!

NATURAL PHILOSOPHY.

1. To what is the weight of a body due? Will two balls of lead, one of 1 lb. the other of 2 lb., in falling from a height, reach the ground in the same time; and if not, why? How could the position of the centre of gravity be found practically in a flat piece of wood of irregular shape?

2. Give drawings and descriptions of two different kinds of pulleys; and determine the relation of the power to the weight in one of them.

3. Three equal weights are hung at the angles of a horizontal triangle; where must a prop be placed to preserve equilibrium? Would this be affected by supposing the triangle itself to have weight?

4. Enunciate the law of motion which states the effect which a force produces on a body in motion. How would a ball of lead dropped from the top-mast of a ship in full sail fall?

5. Define specific gravity. State how the s. g. may be found of oil, of cork, and of lead.

6. Explain the action of the siphon. What is the highest point at which the bend of the siphon may be situated above the surface of the water; and why?

7. A cubic foot of water weighs 1000 ounces av. Living animals have been dredged from a depth of three miles; what pressure in tons per square inch have they lived under?

Part Third.

PERISCOPE.

THE STRUCTURE OF TENDON.—There is, in the last number of the *Med. Jahrbücher* of Vienna, a communication by Herr Spina, on the structure of tendon, which contributes an important addition to our knowledge. The following is a brief abstract of the

most important parts of Herr Spina's article:—Since the publication by Henle and Ranvier of the results of their investigations into the structure of tendon, the conceptions which have been almost universally held on the subject may be thus summed up. Tendon consists of connective-tissue fibrillæ arranged in parallel and fine fascicles. These fascicles are partially or completely embraced by sheaths of a substance that does not swell when in contact with acids. Occasionally, several of these fascicles combine to form stronger capsulated bundles. In the inter-fascicular spaces are cells which, according to Ranvier, combine to form hollow cylinders; according to others, rows of solid cells. Further, elastic fibres exist which run parallel to the connective-tissue fibrillæ.

According to Herr Spina, the development of tendon is to be considered in three stages.

First Stage.—Tendon consists of thin fibrillary fascicles, between which run rows of solid cells. The cells are embraced by a substance that does not swell with acids, and refracts light strongly. When teased it appears in the form of thorn-shaped processes. A transverse section shows it in the form of a ring, which surrounds the cell and sends out membrane-like processes which separate the connective-tissue fascicles from each other. Longitudinal sections show this substance as a ring which surrounds the ellipsoid cells.

Second Stage.—The connective-tissue fascicles have become thicker, and the rows of cells have become longer. The cells have become flattened to a domino-shape, and towards the ends of the row they are seen to be gradually narrower, so that the row is pointed in shape at its extremities. The substance enclosing the cells has acquired a ladder-shape, the sides of which, at the extremities of the cell-row, are continued in a single line. But, besides, threads of the same substance are seen stretching along the middle line of the cell-row. These cannot be considered as being anything else than the thickened cell-wall. These terminal and median threads are to be identified as elastic fibres.

The rows of cells are no longer perfectly parallel, but come into contact with each other; the threads of one cell-row often becoming continuous with those of another. The sheaths of the connective-tissue bundles, visible only in transverse sections, appear in this stage also as membranous processes of the cell-wall.

Third Stage.—The rows of cells have changed to flat bands, some of which are striated longitudinally and transversely; others are non-striated. The striated appearance is produced by elastic matter which persists in the above-mentioned ladder-form. These threads can be traced a long way into the fibrillary tissue, and can be diagnosed as elastic fibres. They form a wide-meshed net which penetrates the connective tissue, the meshes being the cause of the known bulgings of the tendon fascicles.

En résumé.—1. The elastic tissue of tendon takes its origin on the surface of the cells. 2. The fine sheaths in tendon are formed

by membrane-like processes of the cell-wall. 3. The cell-rows become striated and non-striated bands.

THE ELECTROLYTIC TREATMENT OF CANCER (*Nefel*).—This paper contains the description of a mode of treatment which, if experience confirms the results stated here, must be considered one of the greatest boons which have been conferred on suffering humanity. It is no less than a cure for cancer. The paper begins with a discussion as to the nature of cancer. Following the school in which he was educated, the author at first looked on malignant disease as primarily a constitutional affection, of which the tumour was merely the local manifestation. He has been, however, convinced, by the arguments of Virchow, that cancer is really at first a purely local disease, and that any generalization which may occur is a secondary result of the infection of the system from the local disease. He treats of the traumatic origin of cancer, and gives two cases in which mammary scirrhus was distinctly traced to the effects of a blow. Coming to treatment, excision is generally of little or no advantage. He has found electrolysis alone useful. Before giving the results of his treatment and the method, he discusses its *modus operandi*; and states it as his belief that the action is not that of a caustic; but that the electric stimulation has some influence on the protoplasm of the cancer-cells, causing them to alter their mode of life. He believes in Waldeyer's views as to the development of cancer, namely, that it always originates from epithelium, and does not distinguish clinically between carcinoma and canceroid. The first case in which he tried the treatment is given in detail, and is of considerable interest. A gentleman had a tumour of the left mamma, with secondary affection of the axillary glands. It was diagnosed as cancer by Nelaton, Moore, Marion Sims, etc., and was finally extirpated. A similar tumour appeared on the right breast; while the glandular enlargements in the left axilla recurred, and the patient became markedly cachectic. This tumour in the right breast, as well as the axillary glands, were treated in the manner to be afterwards described; and the result was that the tumour completely disappeared, and the cachexia was entirely recovered from. A variety of other cases are given, all showing how speedily many cancers can be made to disappear, and especially how, after the first electrolysis, the lancinating pain passes off. In accordance with Virchow's views, he believes that there is an initial stage in which cancer is purely local, and in this stage he asserts that it can be completely cured by electrolysis. When the disease has advanced beyond this the cure is difficult, and its success depends on whether all the affected parts can be reached. When the tumour is adherent to the deeper parts, or when distant organs are secondarily affected, the treatment is generally unsuccessful. Even in the most aggravated cases, however, this is the best means of reducing the pain, and improving the general condition of th

patient. The method of applying the agent is described with great care. Suppose, in the first place, that the tumour is of moderate size, hard, and slow of growth. With or without chloroform, as circumstances may determine, two, three, or four needles are introduced at equal distances into the tumour, and connected with the negative pole or cathode of the battery. The positive pole or anode is connected with a broad plate, which is applied with a wet compress to the skin at a certain distance from the tumour, and moved from place to place. The current is gradually increased up to thirty-five or forty elements, and after twenty to thirty minutes, gradually lessened, and needle after needle withdrawn. This operation is repeated for a limited number of times at intervals of three or four days, the needles being introduced at different parts of the tumour. After this energetic treatment, a weak current of four to eight elements is to be used daily for months. The negative pole, formed of a flat plate and a wet compress, is placed over the needle punctures, and the positive at a distance from the tumour as before. The sloughs produced by the needles come away after a time, and their track serves as a conductor for the current. If, in the second place, the tumour be a large one, then the only modification is that, during the operation, needles should be extracted and placed in different situations. The number of the needles should not, however, be increased above four at one time. If, in the third place, the tumour be large and ulcerating, then an attempt is made to produce sloughing of it. In this case, the anode (or positive) needle is thrust into the centre of the tumour, and its base transfixed with a number of needles connected with the negative pole. These are left for about twenty minutes, and then inserted in a different position. In from seven to ten days the whole or part of the tumour generally sloughs away, and if it is not entirely removed, the operation may be repeated. After this rather severe treatment, there is never any fever or special constitutional disturbance. In this case, also, after the tumour has sloughed, the treatment with weak currents should be continued for months. Of late, the author has sometimes introduced a slight difference in the treatment of the first and second class of cases. Instead of inserting cathode needles throughout, he has occasionally begun with an anode needle, which he has only kept in for a short time, and he thinks this an improvement on the original plan. The anode needle should only be kept in for a short time, as otherwise it is apt to cause sloughing; after this single application of the positive needle, he continues as before. It is to be noted that the author attaches great importance to the efficient carrying out of the treatment with weak currents, after the energetic currents have been used preliminarily; and he generally entrusts this latter treatment to the patient. The author has also used a similar mode of treatment, as others have done, in aneurisms, varix, etc. His method is in this respect different from the usual, that he does not insulate the stems of the needles which he introduces.—*Virchow's Archiv, and Glasgow Med. Journal.*

Part Fourth.

MEDICAL NEWS.

ACADEMIE DE MEDECINE DE LA FRANCE.—At the meeting on the 9th ult., Professor J. Hughes Bennett was elected Foreign Corresponding Member in the section for Anatomy and Physiology. This, the blue ribbon of medical science, the most distinguished honour that can be conferred upon any medical man, has been well won by the devotion of a lifetime to the advancement of medical science. We heartily congratulate our fellow-citizen on this public recognition of his services to medicine, and trust he may be long spared to enjoy the honour that has been bestowed upon him.

RETURNING LARGE PROFESSIONAL FEES.—It is not often that a professional man has the opportunity, or perhaps feels equal to the effort, of returning two fees of a thousand guineas each in the course of the same year. It is understood, however, that this has occurred with Sir Henry Thompson this year. The first occasion was in connexion with the illness of the Emperor Napoleon III., when Sir Henry, in the most delicate and kindly manner, returned half of his fee of two thousand guineas, and the second is an instance yet more highly honourable to professional delicacy. Called to the bedside of a wealthy friend in the Highlands, near whose shooting lodge he was passing his vacation, he spent nine days in unremitting attendance. At the end a cheque of a thousand guineas was handed to him, but was firmly declined, on the ground that the attendance was given to a friend and during a holiday.—*London Medical Record*.

[The above paragraph savours a good deal more of the sound of the trumpet than of that unostentatious Christian charity which lets not the left hand know what the right hand doeth. And we are told who it is that blows the trumpet (Matt. vi. 2). There are certainly few medical men who could afford to give away a thousand guineas, and the circumstances under which, in the interests of the profession, this ought to be done, are still more rare. We trust our Highland compatriot, who was also in the neighbourhood, and not "on holiday," had at least the refusal of a similar cheque.]

UNDER present circumstances, it may not be out of place to refer the readers of the *Journal* to a paper suggesting the use of the Calabar bean in Cholera Asiatica, by Dr William Munro, in the *Edinburgh Medical Journal* of October 1871.

FALSE TEETH caused the death of Cuvier, and were a source of discomfiture to Lord Brougham. Cuvier, impatient at the interruptions of that perpetual interrupter, M. Glais-Bizoin, in the National Assembly, rose so impatiently to answer him that he jerked his teeth out on the floor, and stooping not less precipitately to pick

them up, fell head foremost, and struck his head against the floor so heavily as to give rise to the illness which proved fatal to him. M. Glais-Bizoin, then a very young man, promised himself to abstain from his fatal habit of incessantly interrupting, but he was incorrigible. Lord Brougham, in the course of the proceedings of a great meeting of the Social Science Association, of which he was president, was stopped in the middle of a speech by his teeth falling out. After groping on the floor, and on presently resuming his speech, he made the best of the incident by observing, "Our teeth are sources of trouble from infancy to old age."

FRECKLES, it is said, are removed by the application, twice a day, of powdered nitre moistened with water.—*Southern Medical Record*.

THE SURVIVAL OF THE FITTEST.—A curious case of the application of the principle of natural selection, under artificial circumstances, is now progressing in our city. As every one knows, until the last year or two we have suffered in Philadelphia from a plague of "measuring-worms," which even Egypt in her palmiest days might have envied.

Some enterprising genius suggested the importation of English sparrows. A diet of worms resulted—a diet which ended our plague. But the pest is coming upon us in a new form. The caterpillar of the moth *Orgyia leucostigma* opens his eyes a little later in the season than his friends the measuring-worms; consequently, for meals he in former times found the luxurious leaf-banquets in our public squares looking like the remains of an over-night club supper, and—didn't prosper.

Thanks to his hairy coat, the sparrows don't take a fancy to him; and, consequently, the measuring-worms being put out of the way, and the leaves allowed to grow, he is prospering and rapidly multiplying under the stimulus of food, good, abundant, and cheap. At present very many trees in this city have again put on the familiar, woe-begone look of old, hiding their misery with the merest tatters and shreds of leaves. "But the new-comer doesn't drop on you!" Doesn't he, though? If he doesn't drop he crawls, or gets on some way or other; and the man who has felt his long hairs tickling his neck, struck for a fly, and found in his hand a bare and *burst*ed carcase, on his shirt-collar a stain, and down his back a bunch of tickling hairs, will vote the "survival of the fittest" in its latest form an unmitigated nuisance.

In order that our readers may avenge us on some of the mothers and fathers of these pests, we add a description of the perfected individuals as they will appear shortly:—Females wingless, or with merely two little wings, like scales, on each shoulder. Males with large ashen wings, crowned by wavy, darker bands on the upper pair, on which, moreover, is a small black spot near the tip, and a minute white crescent near the outer hind angle.—*Philadelphia Medical Times*, July 1873.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Hæmorrhage that occurs during the Continuance of Pregnancy in Cases of Placenta Prævia.* By J. MATTHEWS DUNCAN, M.D.

THIS accident is common, but many cases of placenta prævia, of all kinds, do not present it, for frequently they go on to the full time, or near it, without any hæmorrhage during the pregnancy, and frequently the occurrence of hæmorrhage before the full time is the immediate precursor of miscarriage, or a consequence of its commencement. It is very desirable to know the relative numerical proportions of these different courses of a pregnancy with placenta prævia of different kinds—how many have hæmorrhage during the continuance of pregnancy, how many have not, how many end in miscarriage.

The hæmorrhages now to be discussed are well known. They may occur once or oftener in the latter months of pregnancy. They are comparatively more frequent as pregnancy is more advanced. They frequently appear to have a monthly periodicity. They are often to a great amount, and sometimes they prove fatal without labour commencing.

Although it is these well-known hæmorrhages that I propose now to discuss, I desire to state my conviction, based on observations, that it is an error to regard these as the only hæmorrhages that occur during the continuance of pregnancy in cases of placenta prævia. They are not the earliest that occur, as is attested by the circumstance that placenta prævia is not unfrequently observed in abortions.¹

Hæmorrhage in the latter months of pregnancy in cases of

¹ See William Hunter's *Anatomy of the Human Gravid Uterus*, Plate xxxiv., Figs. 3 and 4. See also Braun Chiari and Spæth's *Klinik der Geburtsh. u. Gynæk.* S. 181; also the Author's *Researches in Obstetrics*, p. 261; also Barnes's *Lectures on Obstetric Operations*, 2d edition, p. 412; also Hecker's *Klinik der Geburtshilfe*, II. Band, S. 170; also Braun, *Lehrbuch der Geburtshilfe*, S. 624, who gives references to Wenzel, D'Outrepont, Busch, Feist, and Thudichum.

placenta prævia may be produced in various ways. So far as I know, it has hitherto been described as being produced only by separation of the placenta; and several theories of the mechanism of this separation—all of them, I believe, erroneous—have been enunciated. For my part, relying partly on my own observations, I believe such hæmorrhage to occur most frequently without any separation of the placenta; though separation does in some cases undoubtedly occur. In those cases of hæmorrhage from placenta prævia which have come under my own observation, I have not found in the placenta those pathological appearances which must, I believe, follow partial separation of one or of several months' standing; and, in the very great majority of cases which I find on record, no mention is made of such pathological conditions of the placenta. These pathological conditions, hereafter to be mentioned, are not such as would easily escape notice, or as would be passed over by observers without description.

There are four ways in which this kind of hæmorrhage may occur:—

1. By rupture of a utero-placental vessel, at or above the internal os uteri.

2. By rupture of a marginal utero-placental sinus within the area of spontaneous premature detachment, when the placenta is inserted, not centrally or covering the internal os, but with a margin at or near the internal os.

3. By partial separation of the placenta, from accidental causes, such as a jerk or fall.

4. By partial separation of the placenta, the consequence of uterine pains producing a small amount of dilatation of the internal os. Such cases may be otherwise described as instances of miscarriage commencing, but arrested at a very early stage.

To the *first* of these modes of hæmorrhage I shall now direct attention; that, namely, in which there is no separation of the placenta, but the blood flows from an opening in an utero-placental vessel. Hæmorrhage of this kind will take place when the placenta is inserted over the os internum, and when, as not rarely happens, the internal os uteri is not at all dilated.

This pathological explanation, it will be observed, quite accords with the absence of all premonitory symptoms, the unexpected character of the bleeding, which observers uniformly describe. Were the bleeding the consequence of some disturbing cause producing separation, whether the cause involved uterine action or not, then it could scarcely be said to occur without any premonition.

When I speak of rupture of a utero-placental vessel, I exclude rupture of the placenta, or rupture extending into the intra-placental maternal blood-spaces. Such ruptures of the placenta do occur, and produce bleeding, not only from the maternal, but also occasionally from the foetal system, some intra-placental branch of an umbilical artery being lacerated and giving issue to blood. It is impossible

to conceive such a rupture, with continuance of the pregnancy, as ending otherwise than in pathological changes of the affected cotyledon or cotyledons, which could not escape the notice of a careful observer when the pregnancy came to an end. In the cases now under discussion, no pathological change in any cotyledon is necessarily produced; and, when the placenta comes to be born, none is to be expected except what is of the most recent kind.

When the placenta is exposed to such violent stretching as to lacerate its whole structure, down to the chorion, or even through it, according to reports of some cases, then it will almost certainly be also separated to some extent; and I shall not refer to this kind of lesion under a separate heading.

So far as I know, there has not yet been made a proper dissection of a case of placenta prævia centralis, but it is much desiderated. In consequence of this anatomical deficiency, we can only guess at the condition of that part of the surface of the placenta which, in cases of this first kind, overlies the internal os of the cervix. For the complete attainment of this desirable object, it will be necessary to secure the body of a woman who has died in advanced pregnancy with placenta prævia, and in whom the connexions of the placenta and uterus have been altogether undisturbed; and it would be best if the woman were destroyed by some disease or accident quite foreign to the genital system. Although the attainment of this is extremely desirable, and for the settlement of some points quite necessary, yet it appears to me probable that a careful anatomical investigation, with the aid of the means of histological research, would discover and identify in the delivered placenta of central, or nearly central, insertion, the spot which, during pregnancy, overlay the internal os of the cervix, and which had no direct connexion with uterine wall; and it may be anticipated that efforts will be made in this direction. The analogous spot may, indeed, be discovered in the membranes even in ordinary cases of delivery.

In the lower animals which have a diffused placenta nearly co-extensive with the whole surface of the membranes, those pieces of chorion which overlie the uterine openings—that is, the internal os uteri and the uterine openings of the Fallopian tubes—are bare of villi, or non-placental. Examples of this are seen in the Balænoptera and Orca dissected by Professor Turner,¹ and in other animals, as the mare. But it is otherwise in woman. These whales and the mare have a thin, diffuse, and very extensive placenta; and while it is quite possible that their whole vascular chorionic surface may act functionally as placenta, it is certain that the villi have a comparatively simple arrangement, and do not even entirely cover the subjacent chorion. In woman, the placenta is concentrated in one small area, and its great thickness, sometimes increased in cases of central placenta prævia, is composed of an aggregated mass of villi,

¹ Transactions of the Royal Society of Edinburgh, session 1870-71, vol. xxvi. p. 479.

whose arrangement is so complex as never yet to have been unravelled. In her, there is no bare chorionic spot where the organ is inserted over the Fallopian tubes or internal os uteri. Indeed, as already said, the placental spot at these situations has never, in cases of this first kind, been described. At the Fallopian tubes its area must be very minute, but at the internal os uteri it must be considerable, covering a surface which is at least of the extent of a square line, and probably much greater.

It is necessary to be very cautious in such a statement as that just made, because we do not know to what extent the attachment of the placenta may restrain or increase the expansion of the internal os. When the placenta is not prævia, the internal os varies greatly in size when near the full time. It is often big enough to transmit two fingers. In a uterus about the sixth month of pregnancy, kindly shown to me by Professor Turner, and in which the mucous plug is still present, and, as he points out, curiously projects into the uterine cavity, the internal os is an oblong opening of a little above half an inch in length, the sides being nearly in contact, but separated by the mucous plug.

This placental area over the os uteri probably strongly resembles the surface of the healthy separated placenta, and consists of decidual structure, the caducous portion of the decidua serotina. It may be called a reflected portion or reflexa of the decidua serotina, for it has a certain analogy with the decidua reflexa. The latter covers the ovum or chorion on the side facing the uterine cavity, while this new or reflected portion of caducous decidua serotina covers the otherwise bare portion of villous chorion on the side of the ovum facing the openings of the Fallopian tubes, or of the canal of the cervix in the uterine wall. Beneath a thin decidual layer, at this spot, will run a portion or portions of utero-placental sinus, conveying maternal blood from adjacent intra-placental cells or caverns.

Sometimes, in woman, an approach to the bare condition of the chorion over the internal os uteri in the whale has been observed. Instances of this kind are well worthy of study, and they may come to be proved to be what they can at present be said only to appear to be. The evidence in favour of there being a tendency to a bare spot is deficient, because of the rarity of the specimens showing it, because so many are seen which do not show a trace of it, and because they are perhaps susceptible of a different explanation, founded on the supposition that they are portions atrophied in consequence of long past separation. Yet, on the other hand, such thinning of the placenta, or absolute bareness of chorion, may appear to be far less frequent than it really is, in consequence, as Hecker suggests, of the rarity of real centrally inserted placenta at or near the full time. It is only after a careful investigation of a sufficient number of abortions in placenta prævia that the question raised can be settled. Meantime, I may be allowed to give some extracts in evidence of its occasional occurrence, especially as it has

important practical bearings, on which, however, I do not here enter.

Hecker¹ makes some interesting remarks on this subject, and describes a very good example of it. He also refers to allied, not quite identical, observations by Küneke and Schuchardt, and describes one of his own, wherein there was placenta succenturiata. The smaller lobe in Küneke's case, in Schuchardt's the membranous junction of the two lobes, overlay the internal os uteri. In Hecker's good example the placenta was composed of two not quite equal lobes, separated by a thinned part; and this two-lobed placenta was so attached over the cervix uteri that the thin part, in which, however, there was evident villous placental tissue, was right over the internal os uteri; and through this thin part, when the lobes were torn asunder, the child was born.

Dr Sirelius, of Helsingfors, has more fully entered on this point than any other author whom I know. I have not had access to his work, but only to a partial republication of it in the *Archives Générales de Médecine*.² His views are in several respects very different from those everywhere entertained. For example, he evidently believes that in all cases of placental insertion over the lower portion of the uterus there is atrophy of the part overlying the internal os of the cervix, which I cannot but regard as an error. It is well known that most cases of central placenta prævia present no such atrophied portion. Besides, were there such an atrophied portion, there would not only be no bleeding before dilatation of the internal os, but there would be no bleeding during the early part of this dilatation—a circumstance quite inconsistent with the clinical history of the great mass of cases of placenta prævia. The same atrophied portions which Sirelius describes as original ovular changes, not the result of separation, are described by Braun as the result of early separation of these parts of the placenta; and in accordance with this condition Braun describes the absence of bleeding during the early part of the dilatation of the cervix. But I see no reason to doubt that the view of Sirelius may be in some instances correct. His description, founded, he says, on an exact analysis of ten cases, is as follows:—"En récapitulant les faits rapportés ci-dessus, on voit que le placenta, se développant sur l'orifice de l'utérus, subit des changements de forme importants; quelquefois, mais rarement, il s'étale en membrane sur presque toute la surface du chorion, ou bien il y a deux placentas entièrement séparés, ou, et c'est ce qui arrive le plus souvent, le placenta est divisé incomplètement en deux par un sillon allant du bord libre jusqu'au milieu. Comme altérations correspondantes, on trouve la membrane inter-utéro-placentaire, incomplètement développée sur les parties situées à l'orifice de l'utérus, et quelquefois remplacée par une couche de tissu connectif, ou par de la caduque en transformation graisseuse, et parsemée de pigment recouvrant une lame mince

¹ Klinik der Geburtskunde, Band II. S. 168.

² Vol. ii. 1861.

de substance placentaire où les villosités, transformées en tissu cellulaire, forment des cotylédons aplatis près de l'orifice de l'utérus, sans veine circulaire. Si le placenta ne se développe pas primitivement sur l'orifice, mais sur les parties latérales, un lobe seulement étant en contact avec l'orifice, on n'y observe pas ces lésions remarquables, mais seulement des amas de fibrine et des apoplexies dans les couches superficielles et dans l'intérieur des villosités." (P. 448.)

But it is common to have hæmorrhage in cases of central or nearly central insertion, occurring in the latter months of pregnancy, and no pathological alterations of the placenta of a corresponding age. Such hæmorrhage is not the result of separation of the placenta. Now, the only explanation which accords with all the facts of the case is that it arises from opening or rupture of a utero-placental vessel overlying the internal os uteri, not from separation of any part of the placenta or laceration of its whole structure. These utero-placental vessels are large trunks, with walls of extreme delicacy or thinness, and they occur at every part of the uterine surface of the placenta, and are very large in the intercotyledonary sulci and in the margin of the placenta. If the opening is formed, there is no difficulty in accounting for hæmorrhage, however great. That such a rupture or opening may occur is proved by the consideration of utero-placental apoplexy, or utero-placental hæmorrhage, there being rupture, first of all, of a utero-placental sinus in such cases. The opening may be produced by violence or increased blood pressure, or it may be the result of some disease of the part.

Such openings are not very rarely seen when the structures are much denser and thicker, as in the bleedings from varicose veins of the leg or of the vulva. Analogous opening of a vein in the broad ligament and hæmorrhage into Douglas's space has been demonstrated by Ollivier d'Angers and others.¹ But the nearest analogy is found in the opening or openings of uterine sinuses observed in a uterine polypus or in the lower part of the surface of a projecting uterine fibroid. Various pathologists have described these openings, and I have myself published cases.² These openings have to be made through the wall of the sinus, and a thin decidua layer in the case of placenta prævia, while in the case of a uterine fibroid there have to be perforated, in addition to the wall of the sinus, a muscular layer, and a layer of mucous membrane.

It is interesting to remark, that in a uterine fibroid or polypus having such vascular rupture or opening, the hæmorrhage does not persistently or constantly flow, but undergoes long periods of arrestment; and this is the case with the floodings of placenta prævia. In both cases the hæmorrhage may prove rapidly fatal.

¹ De l'Hæmatocele rétroutérine, etc. Par le Dr Auguste Voisin. P. 76. Paris, 1860.

² See my paper on Hæmorrhage from Fibrous Tumour of the Uterus, Edinburgh Medical Journal for January 1867. See also Edinburgh Medical Journal, April 1869, p. 955; and, in connexion with this subject, see Leopold's case of Cavernous Uterine Fibroid. Archiv. d. Heilk., Band xiv. Heft 5.

Further, it is interesting to observe that, in a uterine fibroid or polypus having such vascular rupture or opening, the hæmorrhage may, in its recurrence, exhibit a monthly periodicity; and this is sometimes the case with the floodings of placenta prævia.

Of the mode of arrestment of this kind of hæmorrhage I know nothing quite certainly. One may imagine that decidual contractions may have something to do with it, for Turner¹ believes he can demonstrate a muscular layer in the caducous part of the serotina. But the arrestment is probably the result of a local or general anæmia.

The *second* cause is rupture of a marginal utero-placental vessel. Nowhere in the placenta are these vessels more largely developed than at its very margin, and blood may flow from a rupture without separating any portion of placental structure, the blood passing directly between the decidua and the uterus, and escaping through the cervix into the vagina.

This origin of hæmorrhage, during the continuance of pregnancy, in cases of placenta prævia, is not available when the placenta is inserted centrally or nearly so; but it may give rise to it when the placental margin is attached anywhere between the internal os and that circle of latitude of the lower uterine hemispheroid which forms the limit of the spontaneously detaching area. It may, indeed, be a source in all positions of placental attachment, as Jacquemier² has pointed out; but it is only when in the position described that it can be properly included under the pathology of placenta prævia. In hæmorrhage from this source there will not be necessarily any dilatation of the internal os uteri, nor any subsequent morbid alteration of a cotyledon or cotyledons, just as in cases of the first source.

Hæmorrhages arising from this source require no special cause to account for them. Like those from the first source, they may be produced by most of those causes which originate accidental hæmorrhage. But the position of the placental insertion will make a woman more liable to their occurrence than in ordinary conditions, from the increased blood-pressure to which the lower part of the uterus is, in many circumstances and frequently, subjected.

The *third* cause of hæmorrhage, during the continuance of pregnancy in placenta prævia, is partial (or even complete) separation from accidental causes, such as a jerk or fall. There can be no reason why a case of placenta prævia should not be liable to the same accidents as cases where the placenta is normally inserted; and there can be no doubt that hæmorrhages are produced in this way in the abnormal condition.

When separation is produced in this way it is not subject to the laws of spontaneous separation produced by expansion of the internal os of the cervix. Any part may be detached.

When separation arises in this accidental way, the laws of ac-

¹ Journal of Anatomy and Physiology, 2d Series. No. XI. November 1872. P. 127.

² Manuel des Accouch., tome ii. p. 229.

cidental hæmorrhage proper will be applicable to it, always excepting such modifications as arise from the internal os uteri being in or near the separated area. Especially, the part separated will, if pregnancy continues sufficiently long, undergo well-known pathological changes, and ultimately become atrophied.

The *fourth* cause of hæmorrhage is partial separation of the placenta, the consequence of uterine pains producing a small amount of dilatation of the internal os. In such cases, miscarriage may be said to have commenced, but has been soon arrested, and pregnancy has gone on.¹

That the fourth cause of hæmorrhage may occur, no one will, I suppose, be disposed to deny; for all the theories now entertained regarding the cause of these hæmorrhages, erroneous as I am sure they all are, assume that partial separation may take place and lead to hæmorrhage without the arrestment of the progress of pregnancy. When the placenta is naturally inserted, it may during pregnancy be partially separated by uterine pains or spasm; hæmorrhage may thus be produced, and yet pregnancy may continue; and I see no reason why this may not happen when the placenta is prævia.

I believe it is now generally admitted, and I am able myself to attest it, that at all periods of pregnancy, from the earliest onwards, the cervix uteri may be opened up to a greater or less extent without the interruption of pregnancy. Indeed, many cases of threatened abortion in early pregnancy are not sufficiently explained without, in addition, supposing detachment of the lower part of the decidua vera; so large and so persistent is the hæmorrhage. Although it is of the nature of a digression, I may add, that in a case of threatened abortion, which eventually reached the full time, and which I observed, the conclusion seemed inevitable, that not only was the lower part of the decidua vera separated from the uterine wall, but that also a piece of it was detached from the remainder and expelled. I arrived at the same conclusion in a case of early abortion of one of twins, when a portion nearly as big as the palm of the hand was detached and expelled, while pregnancy with the remaining twin went on.

Separation produced in this way is subject to the laws of spontaneous separation of the placenta, produced by expansion of the internal os of the cervix. The first part detached will always be that nearest the internal os, and the rest in regular subsequent order according to position.

¹ It is worth while to note here a possible cause of hæmorrhage to which I have no other opportunity of referring, and which may produce it if the internal os is considerably enlarged during pregnancy with placenta prævia centralis. In such circumstances, a spasm of the internal os may be to such an extent as to produce separation, the spasm of the circular fibres producing detaching expansion. The detaching expansion in this case will be produced by the circular fibres of the lowest part of the body of the uterus, not by the longitudinal uterine fibres, as in ordinary detaching expansion in early labour in cases of placenta prævia. The direction of the expanding force will in this case be the opposite of that in ordinary cases.

The parts of placenta separated in this way will undergo the pathological changes which take place in portions of placenta accidentally separated in any position during pregnancy, and which Gendrin and Simpson have described as the result of separation in quite a different manner. They will, first of all, be thrombosed; subsequently decolorization will take place; and, lastly, thinning and drying, or atrophy, will occur if the pregnancy continues long enough after the separation.

The anatomical source of the hæmorrhage and the arrestment of it, in cases where it is produced by this and the last-described causes, I do not here enter upon, as the discussion of these matters is fully taken up when the subject of hæmorrhage during labour is considered.

Many authors, among whom are Desormeaux and Dubois,¹ Simpson,² Chowne,³ and Murphy,⁴ describe the hæmorrhage in cases of placenta prævia as being a hæmorrhage by retrogression or by regurgitation. I know of no great hæmorrhage which takes place in this manner, and I know no reason for regarding this kind of hæmorrhage as following such a course. The hæmorrhage, when the result of a rupture of an utero-placental vessel, as described under the first two categories, resembles any venous hæmorrhage, and involves no retrogression or regurgitation. When the hæmorrhage is caused by separation of the placenta, either before or during labour, it is not a hæmorrhage by retrogression. It is more truly described as an oozing on a great, sometimes a vast, scale. The uterine sinuses, or these along with the utero-placental sinuses, form a network of elephantine vessels, which combined constitute a great reservoir into which the uterine arteries pour a copious supply of blood, and from one or from numerous openings in this great reservoir the blood is discharged. When a lake is formed in the course of a river, this lake may be tapped and the water drawn off without any regurgitation or retrogression. In this case the network of the uterine and utero-placental sinuses is the lake in the simile.

The explanation which has, in this paper, been given of the hæmorrhage in placenta prævia during the continuance of pregnancy, I have for many years expounded in my annual course of lectures, and I know of no other which is in accord with the facts of the case. Indeed, I can conceive no other, unless it be possible that a healing reunion and return to the previous healthy condition can take place after partial separation of the placenta. I see no reason to doubt the possibility of such healing reunion and return to the previous condition of health, except the very important one that, while clinical experience offers no reliable example of it, or even reason to suppose its occurrence, it offers innumerable examples of failure to reunite, with the invariable consequence of pathological

¹ Dictionnaire de Médecine, 2de edit., tome xix. p. 654.

² Selected Obstetrical Works, vol. i. p. 236.

³ Lancet, vol. ii. 1847, p. 250.

⁴ Lectures on the Principles and Practice of Midwifery, 2d edit., p. 420.

changes in the separated portions of the placenta, ending in atrophy if sufficient time elapses. This curious subject has been noticed by Mauriceau, who asserts that the "secundine being once loosened, . . . never joins again to the womb."¹ Gendrin also discusses it in the second volume of his *Traité Philosophique de Médecine Pratique*, and arrives at what must be regarded as sound conclusions. His opinion may be stated as follows:—that, while healing reunion never occurs, or probably never occurs, it is quite possible between the placenta and the disjoined part of the body of the uterus where it had been previously attached, but that it is impossible that it should take place between the separated placenta and the surface of the expanded cervix, with which it might come to lie in close contact; for Gendrin held the old theory of the production of hæmorrhage, during the continuance of pregnancy, by expansion of the cervix from above downwards during the latter half of the continuance of this state. But neither of these authors has remarked that healing reunion, which alone they discuss, does not imply restoration of the circulation in the separated placental cotyledon or cotyledons; and it must be admitted that this constitutes a great additional pathological difficulty in the way of admitting reunion as a means of substantiating other causes of hæmorrhage besides those already described.

The old explanation of unavoidable hæmorrhage during pregnancy was based on the erroneous belief that the cervix began, about the middle of pregnancy, to be expanded from above downwards, so that its cavity might contribute to form the lower part of the general uterine cavity, and that this expansion gradually progressed during the latter half of pregnancy. With the destruction of this erroneous belief, the theory of course simultaneously fell; or rather, it ought to have fallen. But, like many errors, it has a lingering vitality, and so late a writer as Simpson is found still adhering to it. Even had the belief been anatomically verified, the theory founded on it would not have been in accord with the facts of the case. For, were this theory true, then hæmorrhage should regularly or invariably occur in the latter half of pregnancy, which is not the case. Were this theory true, then there should be, in every case of placenta prævia centralis at or near the full time, a morbid and probably atrophied portion of placenta when the separation, during the continuance of pregnancy, took place, which is also not the case. Besides, this theory leaves those bleedings unaccounted for which take place before dilatation of the internal os has begun.

This theory was held by Gendrin, and accordingly he carefully describes the morbid appearances observed in the placenta, giving also an account of how to investigate recent specimens. His words are as follows:²—"Les altérations anatomiques que les auteurs ont

¹ Diseases of Women with Child, etc. Chamberlen's Translation, p. 87.

² *Traité Philosophique de Médecine Pratique*, tome ii. p. 216.

signalées dans le placenta, lorsqu'il était greffé sur l'orifice interne du col de l'utérus, ne sont point en rapport avec toutes les circonstances que présentent les hémorrhagies qui se rattachent à cette disposition anormale. Elles consisteraient toujours, suivant les descriptions qu'ils en ont données, dans un état comme de dessèchement et d'atrophie de la portion du placenta implantée sur l'orifice interne du col de l'utérus, avec la présence sur l'arrière-faix d'une tuméfaction mamelonnée correspondant à l'orifice du col, vers lequel la portion du placenta qui le recouvre se trouverait poussée et engagée au moment du travail de l'accouchement.

“ Les altérations du placenta, dans les cas d'implantation anormale, ne sont ni aussi simples, ni aussi invariables ; elles se modifient de différentes manières qui se trouvent en rapport avec les diverses formes des accidents hémorrhagiques.

“ L'altération la plus simple que présente le placenta, lorsqu'il a été inséré par un segment peu étendu de son bord sur l'orifice utérin, est la suivante : son tissu spongieux est affaissé, comme déprimé et converti en un tissu homogène rougeâtre, dans lequel du sang est immédiatement incorporé, et qui ressemble assez bien pour l'aspect au parenchyme du poumon refoulé dans la gouttière vertébrale après la pleurésie, mais avec une fragilité qui se rapproche de celle de la rate. Le chorion tantôt s'étend sur cette partie du placenta sans présenter de modification apparente, tantôt il est rompu transversalement et détaché à la limite de la portion altérée et des parties saines continues. Cette rupture nous a toujours semblé avoir été produite dans le travail de l'accouchement par l'interposition nécessaire du segment décollé du placenta entre la tête ou le siège du fœtus et le bord du col. Cette interposition produit aussi très probablement, au moins en grande partie, la dépression de la portion du placenta engagée dans le col. Quand le chorion n'a pas abandonné la partie du placenta qui se trouvait décollée, on le trouve dans presque tous les cas rompu à quelques lignes du bord externe. L'altération placentaire que nous venons de décrire est celle qui se rencontre quand l'hémorrhagie a précédé de peu de temps ou accompagné le travail de la parturition.

“ Dans les cas où des hémorrhagies se sont reproduites à différentes fois et par intervalles plus ou moins éloignés, avant l'accouchement et au moment du commencement du travail, le segment du placenta inséré sur le col est plus étendu et les altérations du bord de cet organe qui a été décollé ne sont pas les mêmes sur tous les points ; il faut alors les considérer sur trois zones excentriques l'une à l'autre.

“ Sur la zone la plus voisine du centre de l'organe, le tissu placentaire est condensé et réduit en un tissu homogène dense, comme granuleux, d'une couleur jaune grisâtre, se rompant avec facilité, traversé par des filaments blanchâtres qui arrivent jusqu'à la surface utérine en se ramifiant, et se perdent dans la couche jaunâtre qui ne présente pas dans son épaisseur le moindre vestige de points rouges.

Au milieu de ce tissu homogène se rencontrent toujours de petits caillots d'un rouge noirâtre, ordinairement très nombreux, qui ne s'isolent pas du tissu homogène environnant, et se fondent dans sa substance. Ces petits caillots pénètrent le plus souvent jusque sous le chorion. La surface du placenta, sur ces parties ainsi altérées, présente souvent des taches blanches d'une à deux lignes de diamètre, très nombreuses et plus ou moins confluentes, faisant un léger relief, et ressemblant au premier aspect aux petites plaques que forme la matière tuberculeuse sur la séreuse abdominale après les péritonites tuberculeuses.

"La zone intermédiaire, excentrique à celle qui vient d'être décrite, présente un tissu rougeâtre dans lequel on reconnaît du sang coagulé, infiltré et même incorporé avec le tissu placentaire. Ce tissu est plus mou et beaucoup plus friable que celui du placenta à l'état sain; son apparence homogène et la continuité de sa trame sont interrompues par des foyers sanguins qui pénètrent à différentes profondeurs de la surface utérine vers la surface fœtale.

"Le chorion, sur les deux zones internes qui viennent d'être décrites, ne nous a jamais offert ni altération de texture, ni déchirure.

"La zone extérieure présente l'état d'altération qui se trouve dans les cas où une petite portion du bord du placenta correspondait à l'orifice du col.

"Il est évident que ces altérations ne diffèrent que par leur ancienneté; elles se sont produites à des époques diverses. Les plus voisines du centre du placenta sont les plus anciennes et correspondent aux premières hémorrhagies, et les plus excentriques aux plus nouvelles: la forme et les limites latérales des altérations le démontrent aussi. L'altération la plus voisine du centre est toujours la plus étroite et la plus éloignée la plus large, en sorte que toute la partie malade du placenta a la forme d'une triangle, dont la base est vers le bord de l'organe.

"Indépendamment de ces altérations l'on trouve sur le placenta des lésions qui ont souvent été produites au moment du travail d'expulsion; elles consistent en des déchirures remplies d'une plus ou moins grande quantité de sang coagulé, que l'on enlève aisément par une macération de quelques heures. Ces ruptures ne sont point transversales, elles ont l'aspect des ruptures, en quelque sorte étoilées, qu'on produit en servant dans la main un corps mou et fragile, de manière à rassembler ses parties périphériques autour d'un centre.

"Les divers degrés d'altération du tissu placentaire, que nous venons de décrire, constituent la première période d'une lésion plus ancienne, que nous avons rencontrée plus fréquemment que toutes les autres sur des arrière-faix expulsées par des femmes qui ont eu à des périodes éloignées de la parturition des hémorrhagies plus ou moins considérables qui ne se sont pas renouvelées. Cette lésion consiste dans la conversion d'une partie du bord du placenta dans toutes son épaisseur, en un tissu homogène, d'une jaune pâle, tirant sur la teinte grisâtre, n'offrant aucune trace de vaisseaux apparents,

La partie du placenta ainsi altérée est constamment au moins de moitié plus mince que les parties saines de l'arrière-faix correspondantes; elle est lisse et d'épaisseur égale dans toute son étendue; les bosselures des mamelons n'y sont plus apparentes. Cette partie de l'arrière-faix contient toujours dans son épaisseur des portions rougeâtres qui ne sont point disposées par couches, mais par grumeaux ramollis, dans quelques uns desquels on reconnaît très bien du sang altéré. Nous avons trouvé dans un cas un seul, et une autre fois deux foyers hémorrhagiques très circonscrits et de six à huit lignes de diamètre, pénétrant et occupant toute l'épaisseur de cette partie du placenta qui avait été insérée sur l'orifice du col et qui était devenue jaunâtre. Le chorion correspondant était d'un blanc mat légèrement jaune; il était épaissi comme par l'addition ou l'infiltration d'une matière d'apparence albumineuse, et l'on ne remarquait point en lui d'injection vasculaire anormale."

Simpson, holding the erroneous theory, and closely following Gendrin, gives an account of what he erroneously calls "successive apoplectic infiltrations," and describes the changes which they would undergo in the latter months of pregnancy. "We can occasionally trace (says he) in the placenta, after its expulsion, different parts of it, showing a series and gradation of pathological changes arising from successive partial detachments."¹ But his description has not the appearance of being derived from the actual examination of any placenta, and certainly he does not describe any. I have never seen such a specimen, nor do I know of any detailed description of one. That such a condition may occur, produced by the fourth cause which I have described, I see no reason to doubt, but it must be very rare. It is to be hoped that obstetricians will direct their attention to placentas born in cases of placenta prævia, with a view to discovering and describing the desiderated specimen.

It is easy to find apoplectic intra-placental clots, utero-placental layers of clotted blood, and thrombosed cotyledons, of various ages and corresponding degrees of decolorization, in the same or in different placentæ; but this is not what is wanted. Thrombosed cotyledon or cotyledons, without or with utero-placental blood-clot, or intra-placental apoplectic clot, must be found in the same placenta, and with due concentric arrangement of different degrees of decolorization, and in such circumstances as dispel doubt as to the cause being successive separations of the placenta around the internal os uteri. This is what is wanted.

When this oldest theory became untenable, another was adopted, based on a closely analogous anatomical supposition—namely, that the lower portion of the uterus became, during the latter half of pregnancy, greatly and specially developed, and that an incompati-

¹ Selected Obstetrical and Gynæcological Works. Edited by J. Watt Black, M.D., etc., p. 223.

bility between the placenta and this part was produced, and thus separation; because the placenta ceased to grow about the sixth month, or at least long before the great development of the lower uterine hemispheroid had ceased. Now, to all this ingenious theorizing fatal objections are easily raised, closely analogous to those raised to the former theory. There is no evidence that the lower uterine hemispheroid is more developed during the latter half of pregnancy than the upper, where the placenta is generally attached. Indeed, I am disposed to think that evidence could easily be found to the contrary effect. Were this theory true, then hæmorrhages should be the invariable rule, or nearly so, in cases of placenta prævia during pregnancy; which is not the case. Were this theory true, there should be in every case of placenta prævia, at or near the full time, an atrophied portion of placenta where the separation took place, which is not the case. Besides, this theory does not account for the separation taking place just at the internal os uteri.¹

Very recently another ingenious theory of this hæmorrhage has been propounded in the graphic pages of Barnes.² This theory may be described as the converse of that which I have last discussed. It alleges that the disproportionate growth is on the part of the placenta, and that the hæmorrhages are the result of loss of relation, or separation produced by the placenta shooting beyond its site. Now the whole of this theory is a hypothetical structure without any basis of facts. But this is far from being all that can be said against it. It is a theory which, if true, should have in support of it the occurrence of similar hæmorrhages when the placenta is inserted in its normal site, for every part of it applies equally well to cases of natural pregnancy. Further, the theory does not explain why the separation should take place at the internal os uteri. It does not explain how, in many cases, there is no hæmorrhage during pregnancy; and, lastly, the fact that no atrophied portion of placenta is found corresponding to the supposed detachment, is hostile to or inconsistent with it.

My theory, as explained in the preceding pages, is that the hæmorrhages during pregnancy are accidental, not necessary, and that their occurrence is favoured by the extraordinary anatomical conditions existing in placenta prævia, as well as by other circumstances, some of which are known, as the increased pressure of the blood above what it would be were the placenta inserted high on the uterine walls.

I believe much harm has arisen from the custom of authors to treat unavoidable as quite distinct from accidental hæmorrhage; whereas their whole pathology, though not identical, is very nearly so; and the study of them is made easier by so regarding them.

¹ For a good account of this theory see the work of Cazeaux, *Traité Théorique et Pratique de l'Art des Accouchements*.

² *Lectures on Obstetric Operations*, etc., p. 401.

ARTICLE II.—*Reduction of Large Herniæ by Means of India-rubber Bands.* By JOHN DUNCAN, Assistant Surgeon, Royal Infirmary.

SEVERAL years ago I heard M. Maisonneuve, in his clinical lectures, referring to the treatment of large herniæ, suggest a method of reducing them which I think worthy of notice. When a hernia of large size is incarcerated or strangulated, it becomes a difficult matter to apply the pressure necessary for its reduction. Plainly the force applied should, if possible, be of such a character that the pressure can be steadily maintained, and must be so adjusted that equal support is given to every portion of the tumour. With the unaided hands this is impossible. One hand must be applied to the neck of the hernia, and under such conditions the other is not large enough to encircle the tumour. The force used is consequently unequal at different points, and therefore dangerous and ineffectual. By means of an indiarubber bandage these indications of treatment may be fulfilled. A turn or two of the bandage is first made pretty firmly round the neck of the sac, and then layers of the bandage are placed in succession over the surface. Care should be taken that the first layer be lightly laid on, so that, until the whole surface of the swelling is covered, the tension of the band may be very slight; but after the first layer one or two others may be applied with a firmer hand. In this way only can complete equality of pressure be attained in every part.

I had recently an opportunity of using this bandage with good effect.

The patient was an elderly gentleman with a large scrotal hernia. His mental condition (one of restless dementia) was such that no truss could be got to command persistently the large orifice. The bowel escaped from the abdomen perpetually. As the orifice was very large, and the danger of strangulation consequently slight, a scrotal bag was deemed the form of apparatus best suited to the case. The bowel was usually returned with great ease when the patient lay down. Once or twice, however, it was not so—the tumour got tense and tender, and pain in the abdomen and vomiting supervened. These attacks would pass off in forty-eight hours if the patient were kept in the recumbent posture. On one of these occasions, when the duration and severity of the illness were greater than usual, I was called to see him. I found the hernia as large as a child's head and very tense. I failed to reduce it. Its unwieldy bulk gave one a feeling of impotence in using the taxis. I procured an ordinary elastic bandage, and, using it in the way I have mentioned, I at once succeeded in returning the bowel.

This method of reduction is of course applicable only to large herniæ. In them only could the bandage be applied. But these cases seem peculiarly suited for the method, inasmuch as the

orifice is commonly large, and the condition therefore calling for interference is primarily rather one of incarceration than strangulation. A considerable amount of force is justifiable under such circumstances, provided always that it can be applied with the precautions necessary to prevent bruising or other injury of the hernial contents. With the hand this is difficult, with the bandage easy; and the degree of force used may be estimated by a preliminary experiment on the closed fist. The bandage may be made of simple indiarubber, or of cotton and indiarubber webbing. The one I used was of the latter material, very soft and elastic.

ARTICLE III.—*Case of Chronic Tetanus cured by the Removal of a Cicatrix and Involved Nerve.* By THOMAS ANNANDALE, F.R.S.E.; Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery.

JOSEPH M'CLUSKEY, æt. 15, cabinetmaker, was admitted into my wards in the Royal Infirmary on the 7th of May 1873, with well-marked symptoms of chronic tetanus.

Fifteen days before his admission, he wounded the sole of his foot with a spike, when climbing over some railings. The wound was thought to be superficial, and did not cause him much pain, but he remained in the house for a week. Two days after resuming his work, he felt for the first time a stiffness in his neck, and an inability to open his mouth properly, and these symptoms increased in intensity up to the time of his admission. Two days after his admission I made a careful examination of his case, and found the following conditions present:—His face presented the characteristic "tetanic grin" very well marked, and his eyelids were partially closed, owing to a contraction of their muscles. The muscles of the neck were hard and contracted, and there was difficulty and pain in moving the head. In assuming the upright position, pain and stiffness were complained of in the muscles of the back. The jaws could only be separated to a slight extent, and the muscles over them felt hard and contracted. There were no spasms of the muscles of the larynx, or of those of respiration, and the patient could swallow freely, except that the stiffened condition of the neck caused the act to be performed somewhat awkwardly. On the sole of the left foot, in the space between the heads of the metatarsal bones of the great and second toes, there was a small cicatrix, about a quarter of an inch in length, the result of the wound caused by the spike. A little hardness could be felt around the cicatrix, which was not tender on pressure, and nothing peculiar could be detected in connexion with it. No pain was complained of in or near the cicatrix.

The bowels having been freely opened by means of a croton-oil

pill, small doses of morphia were given every two hours. As three days of this treatment was followed by no relief to the symptoms, thirty grains of chloral three times a day were substituted for the morphia. Only very slight improvement took place after two days of this remedy, and for the first time the patient began to suffer from slight spasmodic contractions of the muscles of the thigh and leg of the injured side. My colleague, Dr Fraser, was good enough at this stage of the case to see the patient with me, and we both considered that the proper course now to be adopted was the removal of the cicatrix and any nerve which might be involved in it; and, if this failed to give relief, I proposed to divide the posterior tibial nerve at the ankle.

On the 19th of May the cicatrix was cut out to a depth of about a quarter of an inch, but finding hardness and discoloration of the tissues still deeper, I dissected out a portion of these deeper textures, so as to take away all the thickened portions. Together with these a bit of nerve three-quarters of an inch in length was removed. The nerve removed consisted of one main branch and two smaller branches, and one of these small branches was found thickened and firmly embedded in the hardened tissues forming the cicatrix. The whole portion of nerve seemed somewhat enlarged, and was certainly congested in appearance. All medical treatment was omitted after the operation.

For the first two days after the operation little change was noted, but on the third day a most marked improvement took place, and from this time the symptoms rapidly disappeared. The patient was discharged cured on the 6th of June. I may mention that the patient's general temperature was carefully observed twice daily, but no alteration of any importance occurred during the progress of the case.

Remarks.—This case is interesting as an example of the chronic form of tetanus following an injury of recent infliction, for it is certainly much more common to meet with the more acute variety in cases such as the one I have related.

The special interest of the case, however, is the satisfactory result which followed the removal of the cicatrix and involved nerve.

On looking over my note-book, I find that during the last few years I have amputated in three cases, twice the forearm, and once the thumb, and divided the median nerve in a fourth case, with the hope of staying the progress of acute tetanus following injury. In none of these cases was the operation successful, but the operation was in all performed some days after the symptoms first showed themselves.

The general experience of practical surgeons in connexion with such operative proceedings in tetanus, has not been very satisfactory. A few cases have been decidedly relieved by the division or removal of portions of nerves, or by amputation, but in the majority of cases no good or permanent result has followed the operation.

Although the attention of surgeons is usually directed to the removal of any local cause of irritation in cases of tetanus, I think we have hitherto erred in some cases in delaying the operative proceedings (more especially the division of nerves, or removal of a portion of them) too long; and I would venture to express the hope that in the future we shall more frequently, and at an earlier period in the progress of cases of tetanus, the result of injury, resort to operations which have for their object the removal of an irritated portion of a nerve, or the cutting off its communication with the spinal cord.

The result of the case just described is, I think, an encouragement in connexion with this subject, and my only regret is that I did not remove the cicatrix and involved nerve earlier in the progress of the disease.

ARTICLE IV.—*Case of Polypus of the Cavity of the Cervix Uteri; with its Microscopical Examination.* By J. R. HARDIE, M.B.

POLYPOUS growths from the cervix uteri have, from their frequency and importance, been the subject of much investigation and description, but their differentiation, according to minute and accurate microscopical characters, has not as yet been nearly thoroughly worked out.

The subjoined case, which occurred in the practice of Dr Matthews Duncan, to whom I am indebted for permission to investigate and report it, seems to me to indicate the great value of a histological examination of such morbid growths in assisting the physician to arrive at an accurate impression as to the probable future of a case of this nature. By the naked eye the true composition of the polypus about to be described could not be distinguished.

Miss K., who is thirty-one years of age, enjoyed uninterrupted good health until about three years ago, when the complaint detailed below first commenced. She began to menstruate when fifteen years old. The discharge was natural in every particular at that time, and continued so until she was twenty-eight years old—that is, three years ago—when it became very profuse in quantity, containing clots of blood. Two years ago she consulted a physician, who, she says, told her that he detected a polypus of the womb, which he removed. This was followed by a slight amount of improvement, but the menorrhagia still continued, the discharge containing blood-clots. During the month of June 1873, Miss K. put herself under the care of Dr Matthews Duncan, who, on examination per vaginam, discovered a polypus growing from the cavity of the cervix uteri; no other disease of the uterus was then to be found. Dr Duncan removed this polypus, seizing it with an uterine forceps, and detaching it by means of a pair of scissors

from its seat of attachment. No blood loss followed the operation, and recovery from it was rapid.

The monthly period immediately following recovery from the operation was profuse, the discharge containing some blood-clots; the succeeding one, however, followed after the usual interval; the quantity of discharge was very much less, and contained only a single small blood-clot. On a vaginal examination being made on the 1st of August 1873, nothing unnatural was to be made out. Miss K.'s general health has greatly improved.

Microscopical Examination.—The tumour was about the size of a walnut, somewhat pyriform in shape, but broadest at the end which was attached to the cervix, becoming narrower towards its free extremity. It measured 3 cm. in length, and its greatest girth was $7\frac{1}{2}$ cm. It was moderately hard and somewhat elastic. The outer surface was smooth, abounding in irregular elevations varying in size, which also exhibited a smooth surface. It was of a pale red colour. On dividing the tumour longitudinally numerous cavities were observed; some of these were of a round shape, others cylindrical and elliptical, with branches proceeding from them. They varied in size from a small pea to a pin-point. In the tissue small masses of extravasated blood were to be seen. There was no juice to be expressed from the tumour. A scraping examined under the microscope exhibited red and colourless blood-corpuscles, cylindrical epithelial cells, and connective-tissue fibres. The tumour, preparatory to a more minute microscopical examination, was immersed in Müller's solution, allowed to remain in it for a week, and then transferred to a vessel containing rectified spirit, where it was kept until hard enough for making microscopical sections.

On microscopical examination of sections cut from the tumour, prepared in the above-mentioned manner, it was seen to consist of fibrillar connective tissue and cellular elements, the former greatly predominating. In the connective tissue many broad bands or bundles of fibres were to be seen lying in various positions; some ran parallel to one another, others intersected and crossed in different directions. The tissue forming these bundles apparently consisted of fusiform cells lying close to one another. On putting a small slice of the tumour into a solution of caustic potash (40 per cent.), and allowing it to remain there for half-an-hour, on being teased out with needles and put under the microscope it was found that the connective tissue had been rendered transparent (was dissolved), and that many of the fusiform cells previously alluded to had been isolated. They presented the characteristic appearances of unstripped muscular fibre cells. On proceeding with the general microscopical examination of sections of the tumour, numerous bloodvessels were observed. These varied in size and shape. Some of them were spiral and possessed thick walls; the transverse arrangement of their muscular coat was distinctly

observed. In others of the bloodvessels the walls were seen to be thin, consisting only in some of a few layers of spindle-shaped cells; their lumen was large, and the lining membrane, consisting of endothelium, was readily recognised. Many of the vessels contained recently coagulated blood. The surface of the tumour was covered with a layer of cylindrical epithelium with well-marked nuclei. This epithelium covered the irregularities described as existing on the surface.

In classifying this tumour, we have to consider the structures of which it is composed, and their relations to one another. We have seen that it consists of fibrous and muscular elements, the former in excess, and of a considerable number of vessels. We must on this account classify it with the fibro-mymata, and, on account of its abounding in vessels and its peculiar manner of growth, add the terms vascular and polypus. The site of origin of the tumour, the cervical canal, the presence of the cylindrical epithelial cells on its free surface, and the similarity of its structure to that of the healthy cervix uteri, leads us to the conclusion that it is a circumscribed hyperplasy of the tissues composing the neck of the womb. On the mucous membrane of the tumour, however, the well-known penniform arrangement, present in the natural cervix, was absent.

The microscopical examination, the general characters of the tumour, and the subsequent history of the patient, indicate the benign nature of the growth.

Explanation of the Plate.

- a. Longitudinal section of the free surface of the tumour, showing the cylindrical epithelium.
- b and c. Longitudinal and transverse sections through bloodvessels.
- d. Appearance presented by a section of the tumour after treatment with caustic potash, as described in the text. Hartnaek; Ocular 4. Objective 7.

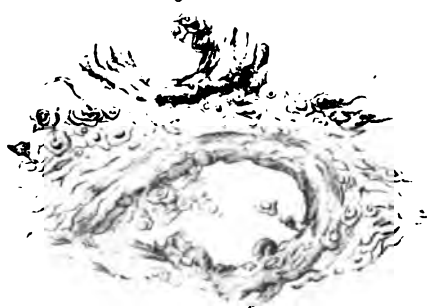
ARTICLE V.—*On the Use and Abuse of Purgatives.* By F. PAGE ATKINSON, M.D., etc., late Surgeon, St Bartholomew's Hospital, Chatham, and Royal South London Dispensary.

ALTHOUGH it is a well-acknowledged fact, that everything is capable of being put to a bad as well as a good use—of being used in excess as well as in moderation; or, in other words, of being abused as well as used,—still we find people, apparently in total ignorance of the harm they are doing, employing things themselves, or causing them to be employed by others, at wrong times or in excessive quantities. For instance: with respect to alcohol, there are some who, from the manner in which they act, seem to believe that “Shallow draughts intoxicate the brain, and drinking deeply sobers them again;” but, sooner or later, the baneful effects show themselves, and preach, in silent though unmistakable language, the folly of excessive indulgence. Again, the physician may, with-

a



b



c



d





out sufficient thought of the consequences, recommend an hysterical patient to take a little stimulant whenever the fit of depression comes on; and the result is that the foundation is, not unfrequently, laid of a disease, more distressing and more certainly fatal than that which originally existed. But the excessive use of purgatives, although producing painfully trying and often dangerous symptoms, has not hitherto attracted the attention one might have naturally supposed, and the consequence is that they are still resorted to on all occasions, not only for the most simple, but also for the most serious disorders.

It is my intention, therefore, in the present paper, to take into consideration the subject of Constipation—to show when purgatives are beneficial, and when injurious; and to lay down some general rules for their administration.

First, then, as regards the causes of constipation: these are—

I. Deficient contraction of the muscular coat of the bowel, owing to prolonged retention of fæces.

II. Deficient nervous and muscular power, occurring after fevers and exhausting diseases, or as one of the results connected with that terrible habit of self-abuse.

III. Intestinal excitement with deficient secretion, consequent on the habitual use of purgatives.

IV. Peritonitis.

The treatment in each of these cases is as plain as possible. In the first mentioned, get the patient to observe stated times for going to stool. If there is a difficulty in obtaining relief at first, give a nerve-tonic, as iron, quinine, nux vomica, etc., in combination with some purgative, as aloes, Epsom salts, compound colocynth powder, etc. A favourite form of mine is, *Extracti nucis vomicæ*, gr. $\frac{1}{2}$; *pulveris ipecacuanhæ*, gr. ss.; *pilulæ colocynthidis* co., gr. iij.; *extracti hyoscyami*, gr. iss. *Fiant pil. ij., p.r.n.s. hora somni.*

Where the patient is suffering from constipation after an attack of fever, or some exhausting disease, as diarrhoea, prescribe rest and support, and bracing air.

In the case of self-abuse, the habit must be stopped, or but little permanent good can be expected.

Where there is intestinal excitement, consequent on the habitual use of purgatives, convince the patient first of all as to the injurious effects, and the non-necessity of purgatives; point out clearly, at the same time, that there may be some little difficulty, and that it may take a little time to get the system in a proper healthy condition; then order a cold sponge-bath every morning on rising, and a wet bandage to be worn round the waist during the day. The diet should be light and nourishing; highly-seasoned meats should be avoided, and light wines should be taken in preference to port and sherry; the mind should be kept diverted as much as possible, and the patient should take a moderate amount of walking or riding exercise daily.

If a purgative should be required at first occasionally, I would recommend the following:—*Extracti aloes socotrin*, gr. ij.; *ferri sulphatis gran.*, gr. i.; *extracti belladonnæ*, gr. $\frac{1}{2}$; to be made into a pill, and taken at bed-time; but get the patient to abstain from using purgatives as much as possible.

Where peritonitis exists, order the patient to keep as quiet as possible—spongiopiline dipped in a decoction of poppy-heads, and camomiles or a linseed poultice, made up with the above, to be constantly applied to the stomach—opium to be given internally, and a diet consisting of beef-tea, milk, jelly, corn-flour, and light-boiled eggs.

Having now considered the causes and treatment of constipation, let us see what symptoms indicate the necessity for the use of purgatives.

Is it necessary, in order to the preservation of health or the recovery from illness, that the bowels should be moved every day? Most undoubtedly not; and, as a proof of this, I can cite three instances at the present time, where, though the persons are in perfect health, the bowels are only moved once a week. During illness nature often makes this effort towards the preservation of strength,—e.g., in quinsy,—and she again sets to work to perform her natural duties, as soon as the strength is sufficient for the purpose. And, again, to show what may be done mechanically, I may mention, that in one case where I thought hectic fever might come on if the patient were at all lowered, I kept the bowels locked up for one month, and the result was an excellent recovery.

See, too, what nature does when the system has become exhausted by diarrhoea or fever: she measures the end by the necessity of keeping all things quiet till the patient is sufficiently strong.

In the commencement of fever, serious harm may be done by the administration of a purgative, for the bowels may get into such an irritable state that nothing will quiet them. This especially holds good with respect to enteric fever.

It must be remembered that purgatives cannot draw off morbid poisons, or get rid of any mechanical obstruction in the bowels, any more than they can remove a clot on or in the brain.

The habitual use of purgatives amongst women I believe to be one of the most fertile causes of hypochondriasis (upsetting, as they must do, the stomach and the solar plexus of the sympathetic), as well as of uterine and intestinal congestion. They are useful when used with discretion in amenorrhoea; dropsy resulting from disease of the heart, liver, or kidneys; sciatica, when dependent on a loaded state of bowel; and in cases where there are dull, heavy, and languid feelings, with a disinclination to mental and bodily exertion, an irritable temper, failing appetite, and coated tongue. The nature of the purgative must depend of course on the nature of the case; but in amenorrhoea, aloes and myrrh pills are the best; in dropsies, the compound jalap powder is of most service; in sciatica, the compound colocynth pill, or the compound decoction of aloes,

may be recommended; in hæmorrhoids, the confection of senna; while in cases of biliousness, a blue pill, followed up by a dose of Epsom salts, appears to give the most ready relief (the blue pill acts on the duodenum, and hurries the bile downwards, while the Epsom salts cause the other part of the bowel to contract, and so evacuate the bile before it has a chance of being re-absorbed into the blood). It often happens that slight biliousness may be got rid of by exercise, a light diet, and a little effervescent saline. Supposing a necessity to exist for the administration of a purgative, it is often a matter of doubt how often the dose should be repeated: the rule I adopt is to repeat it once, and if after this there is no action, to give a copious warm-water enema. This is safe practice, and the desired result is almost always obtained at once. I recollect on one occasion being consulted by a fellow-practitioner regarding a case where a succession of purgatives had been given without any effect, for a supposed case of stoppage, and the patient was said to be sinking. I advised my friend to order fomentations to the stomach, and a full dose of laudanum. This he did, and the patient began to recover from that moment, and eventually got perfectly well. From all I can see, I would say the less we make use of purgatives the better. Nature knows her own work; and if we take regular mental and bodily exercise, eat and drink moderately, we shall find this as a rule quite sufficient for keeping us in good sound health, and also for preserving a "*mens sana in corpore sano*."

ARTICLE VI.—*On the Action and Sounds of the Heart.* By
GEORGE PATON, M.D.

PHYSIOLOGISTS have been accustomed to examine the action of the heart in cold-blooded animals, to determine the laws that regulate its movements. Dr Hope performed a number of experiments on frogs and on turtles, to perceive the manner in which the auricle and ventricle contract and dilate; and Professor Muller, of Berlin, from the observations he made on the movement of the heart in frogs, states a general law respecting the rhythm of the heart. Oesterreicher¹ placed a body on the heart of a frog heavy enough to press it flat, but sufficiently small to allow the heart to be observed, and it was seen that the body was lifted during the contraction of the heart, but that during its extension it remained flat; from which fact he concluded that the extension of the heart, after its contraction, is not a muscular act; and Dr John Reid endorses the opinion as a legitimate deduction applicable to warm-blooded animals; and physiologists still continue to pursue their investigations on the action of the heart in cold-blooded animals.

We make these statements to show that physiologists have considered the action of the heart to be maintained on the same principle

¹ Muller's *Handbuch der Physiologie des Menschen*, Erster Band, p. 163.

in cold as in warm blooded animals. And convinced of the truth of this doctrine, we concluded that, if the heart pulsate in the same manner in cold as in warm blooded animals, the sounds produced must be of the same character, and ought to be equally relied on. We therefore instituted a series of experiments on American turtles, during the highest temperature of the season, when the action of the heart is most vigorous, and the auricle assumes the physiological condition of a warm-blooded animal; and we determined, with the greatest care, the manner in which the sounds of the heart are produced.

We have shown, in the course of our experiments,¹ that when the ventricle contracts, and sends the wave of blood into the aorta, it distends its walls, which, being very elastic, react with energy, and the blood recoils against the semilunar valves and closes the orifice as the wave is impelled along the tube, the impulse of the aorta being synchronous with the contraction of the ventricle; and the first sound of the heart is heard most distinctly at the origin of the aorta, over the semilunar valves, and is produced by the force with which the blood is propelled through the aortic foramen, and recoils against the valves, closing the orifice as the aorta pulsates. Correctly speaking, the blood does not fall back against the valves, as in a rigid and unyielding tube, but the distended aorta, in giving the impulse to the blood-wave by which the valves are closed, reacts from its origin, rendering the arch more curved and tense as the blood is sent along it. Hence the first sound is dull and prolonged, synchronous with the contraction of the ventricle and impulse of the aorta. And the second sound is produced by contraction of the auricles, and is heard most distinctly over the ventricular foramen, as the blood is sent with force into the ventricle distending its walls. It closely follows the first sound, because, during the vigorous action of the heart, the auricles contract immediately after the arteries pulsate.

The doctrines, then, which we maintain are, 1st, That the reaction of the aorta takes place in connexion with the contraction of the ventricle; 2d, That the blood in the distended aorta recoils against the semilunar valves, and closes the orifice as the aortic impulse is imparted to the wave; and, 3d, That the sound produced by the blood recoiling against the semilunar valves terminates the first sound of the heart.

These statements we consider applicable to the action of the heart in all vertebrated animals.

Dr Hope, in the course of his experiments on the denuded heart of an ass, found that a sound is produced by the blood recoiling against the sigmoid valves and closing them. But he believed this took place during the diastole of the ventricle, and was produced by a column of blood in the aorta and pulmonary artery

¹ Researches on the Action and Sounds of the Heart. J. and A. Churchill. 1873.

falling back against the valves when the propelling force of the ventricle was withdrawn. And this would have been the case if the ventricle had impelled the blood along rigid and unyielding tubes, for the blood in the aorta would have fallen back against the valves and closed them when the walls of the ventricle relaxed. But the aorta is distensible and elastic, and on being distended by the ventricle, instantly reacts and forces the blood back against the valves and closes them as it is impelled along the arch, the ventricle being still contracted. And the view that the elasticity of the distended aorta forces the blood against the valves, and shuts them during the ventricular diastole, is opposed to the fact that the pulsation of the aorta is synchronous with the contraction of the ventricle, and shortly precedes the pulse at the wrist and the extremity of the arteries, which can be felt before the second sound commences.

The question, then, which we have to decide is, When are the sigmoid valves closed?

"Les parois sont très élastiques; lorsque une ondée de sang est projetée dans l'aorte par la contraction du ventricule, elles cèdent à la pression ainsi exercée, comme le ferait un ressort; mais elles tendent ensuite à revenir sur elles-mêmes, et à chasser le sang qui les distend." ¹ And in all our experiments on the heart, whether in cold or in warm blooded animals—for we have frequently observed the action of the denuded heart in both—we have perceived that the distended aorta instantly reacts, forcing the blood against the valves, and closing the orifice in impelling the wave onward; and the ventricle could not propel the blood in successive waves along an elastic tube like the aorta without producing this result. If the walls of the aorta yield to the pressure of the blood-wave from the ventricle on being fully distended, they must react with force, and in reacting throw the blood against the valves, and close them in sending the wave onward, the impulse of the aorta being synchronous with the ventricular systole, and slightly preceding the pulse at the extremity of the arteries.

These facts enable us to arrive at the general conclusion that the sigmoid valves are placed at the mouth of the aorta, to aid and assist in the propulsion of the blood along a distensible and elastic tube. The ventricle distends the walls of the aorta in sending the blood-wave with force into it. The aorta, in virtue of its elastic power, instantly reacts and impels onward the wave; the impulse of the ventricle is transferred to the aorta, and the aorta, in reacting, completes the systole, the valves becoming the point of resistance against which the blood recoils on being forced along the tube; and the ventricle is contracted behind the valves, as if supporting them, when the aortic impulse takes place, the aorta, according to Poiseuille, always reacting with greater force than that by which it is excited, ² thence, in impelling the blood along the large arteries, the

¹ Zoologie, p. 70, par M. Milne-Edwards. ² Carpenter's Physiology, p. 432.

sigmoid valves are closed, and not when the ventricle dilates behind them.

It seems difficult for physiologists to believe that the blood in the distended aorta reacts against the semilunar valves, and closes them as the wave is sent along the tube. But the ventricle, when it contracts, closes the auriculo-ventricular valves, by the blood being forced back against them when it is propelled into the aorta; and the moment the contractile impulse of the ventricle is finished, the distended aorta, which is very elastic, reacts, causing the blood to recoil against the sigmoid valves, and close them as the wave is impelled onward.

We therefore consider the view untenable which supposes that the sound produced by the blood recoiling against the aortic valves, and closing the orifice, is the second sound of the heart, and coincident with the diastole of the ventricle. For the valves are closed, as we have seen, in connexion with the contraction of the ventricle as the aortic impulse takes place, which precedes the pulse at the wrist. And the sound heard constitutes the termination of the first sound of the heart, which is a dull and prolonged sound, and produced by the force with which the blood is propelled through the aortic foramen, and the valves close as the aorta pulsates. When the diastole of the ventricle commences, its walls relax, and, in unison with it, the parietes of the aorta become soft and compressible, which is a state of rest; and at the moment there is no stretching or expansion of the sigmoid valves, nor falling back of the blood against them to produce a sound. And the experiment of the hooking back of the sigmoid valves, adduced in support of that view, produces exactly the same result as is so frequently observed in the case of disease of the valves, when they are rendered incompetent. The first sound of the heart, we have seen, consists of two parts; the first part produced by the blood being sent with force through the aortic foramen, and the second part by the blood of the distended aorta recoiling against the sigmoid valves and closing them; and when one of the valves is hooked back, this portion of the sound is converted into a hissing, and, when the valves are rendered incompetent, into a regurgitant murmur. But this does not represent the second sound of the heart, as it occurs when the blood-wave proceeds along the aorta to produce the pulse at the wrist. A portion of the blood is forced back into the ventricle as the aortic impulse is imparted; and in consequence of this regurgitation the arteries are unfilled, not sufficiently distended, and the pulse "sudden, without any prolonged swell of the arteries." And the reason why the second sound of the heart is not then heard is, that, the ventricle being partly filled by the regurgitation of the blood, the auricles do not contract with enough energy in sending the blood through the auriculo-ventricular foramen to produce a distinct sound.

And this condition of incompetency in the aortic valves is produced from the stress to which they are subjected when the

distended aorta reacts with much force in propelling the blood-wave onward. If an obstacle exists to the transmission of the blood along the large arteries, or through the arterial system, the ventricle exerts increased action to overcome it, in consequence of which it distends the aorta with greater force; and the walls of the aorta, reacting with increased energy, throw the blood against the valves with a power which they cannot sustain, and regurgitation of a portion of blood is produced into the ventricle. But this takes place as the blood-wave is sent along the aorta during the arterial systole, and not during the ventricular diastole.

In further proof of the fact that the blood-wave in the aorta recoils against the semilunar valves, and closes them in connexion with the ventricular systole, we may observe that in some animals, as the American turtle, there is an additional layer of circular fibres placed at the origin of the aorta, which fall back behind the valves as if to sustain them more when the blood in the distended aorta recoils against them as it is impelled along the tube. And in the American sturgeon a layer of muscular fibres are placed or extended along the origin of the aorta, in connexion with the muscular fibres of the ventricle, and when the ventricle contracts, the aorta contracts simultaneously with it. Of this fact there can be no doubt. We have frequently observed the action of the denuded heart in these animals, when respiration was maintained with considerable vigour; and we perceived that, when the ventricle contracted and sent the blood into the aorta, the origin of the aorta contracted in immediate connexion with the ventricle, and with much energy, as if that were the point from which great force proceeded, the origin of the aorta being apparently pushed forward, as if giving an impulse to the blood along the arch; and when the walls of the ventricle relaxed on contraction of the auricle, the parietes of the aorta relaxed in unison with them. Till again distended by the blood from the ventricle, the movements were repeated.

We have here distinct corroborative proof of a general law that exists in all vertebrated animals in which we have examined the action of the heart, viz., that the aorta pulsates or contracts simultaneously with the contraction of the ventricle closing the semilunar valves, in impelling the blood-wave along the arch; and in these animals there is a double tier of semilunar valves, as if to resist the greater force of the contraction of the aorta, and a single tier a little higher in the arch, to sustain its impulse on the wave.

Now, should the aortic valves be rendered incompetent, a portion of blood would be thrown back, or regurgitate into the ventricle, as the aorta impels the wave along the large arteries; so that, in disease of the sigmoid valves, it is during the ventricular systole and aortic impulse that the regurgitant reversion is produced, and not during the diastole of the ventricle.

We have based our views of the sounds of the heart on the sounds that are heard in the denuded hearts of American turtles during the

highest temperature of the season, when the action was maintained with great vigour at forty or forty-two pulsations per minute; and we have stated, as admitted by physiologists, that the ventricle contracts and the aorta pulsates in precisely the same manner in cold as in warm blooded animals—in the turtle as in the ass. And why should not the sounds of the heart be admitted to depend on the same principle in both? All the elements supposed to constitute and produce sound are present in the one case as in the other. The muscular walls of the ventricle contract with energy, and send the blood with force through the aortic foramen, and the blood recoils against the valves by the vigorous impulse of the aorta. And the sounds heard are often so clear and distinct that nothing can be more satisfactory, and bear the closest resemblance to the sounds of the heart in man and mammalia. To object to these sounds is to assume that vertebrated animals are not all constructed on the same type, and yet the only difference is, that the auricles pour the blood into one ventricle, which renders it not so highly arterialized as in the left side of the heart of mammalia, but more so than in the right, which is venous blood. And still the sound produced on contraction of the right ventricle is equally relied on, as that produced on contraction of the left.

Cruveilhier made observations on the action and sounds of the heart in the case of an infant only nine hours old, whose heart had protruded without the thorax, having passed through an opening in the upper part of the sternum; and Dr Carpenter adopted the data derived from the observation of Cruveilhier as the basis for several of his conclusions respecting the action and the cause of the sounds; and physiologists have not objected to the principle of his argument, though they may differ from him respecting the manner in which the sounds are produced. But Billard¹ informs us that he found, in examining the bodies of a great many infants who had died within a few days after birth, the foramen ovale and other circulatory passages peculiar to the fœtus were generally closed about the eighth day; and that in nineteen infants who had lived only one day, the foramen ovale was completely open in fourteen. Milne-Edwards also states—"Dans le premier temps de la vie, tous les animaux à sang chaud se rapprochent plus aux moins les animaux à sang froid;"² so that we may conclude, the infant on whose heart Cruveilhier made his observations partook much of the character of cold-blooded.

We consider, then, that we have proceeded on sound physiological principles, in the series of experiments that we instituted on the denuded heart of turtles, during the highest temperature of the season, to determine the cause of the sounds of the heart; and we are decidedly of opinion that these animals afford a much better field for investigating the action of the heart, and arriving at a correct

¹ *Traité des Maladies des Enfants Nouveau-nées*, p. 557.

² *Zoologie*, p. 117, par Milne-Edwards.

knowledge of the cause of the sounds, than is obtained from the denuded heart of warm-blooded animals. In the warm-blooded, as in the dog and the ass, the operation of laying open the thorax and denuding the heart produces a great shock on the system. And the fact that you require to maintain artificial respiration to continue your investigations for any length of time, interferes materially with the action of the heart, rendering it weak and irregular, and the sounds indistinctly heard. But in turtles, the effect of the operation on the action of the denuded heart appears but slight. If the temperature of the day be high, the heart continues to pulsate with energy, and in a normal and regular manner, after being exposed; and the animal will survive for several days, affording an ample field to observe the action and investigate the cause of the sounds.

The auricles contract with vigour, as in warm-blooded animals, distending the ventricle, and the ventricle sends the blood with force into the aorta, exciting its impulse on the wave as it is sent along the arch. But in listening to the sounds of the heart, the action appears reversed; the dull and prolonged sound which is produced during contraction of the ventricle and pulsation of the aorta, is recognised as the first sound of the heart; and the short and acute sound, which depends on contraction of the auricles, appears the second sound of the heart, closely following the first. But these facts could not have been determined without carefully observing the action of the denuded heart as we listened to the sounds; and it was distinctly ascertained in the course of these experiments, that the first sound of the heart represents the termination of the beat, and the second sound the commencement of a new beat, which seems to follow the other as an immediate sequence, but is in reality the first sound of the heart.

We conclude, then, that the sounds heard in the denuded heart of turtles depend on the same principle as the sounds produced in the heart of other vertebrated animals, and are equally to be relied on as the sounds heard in the heart of the dog, the ass, the horse, and man.

From these statements, it appears, physiologists are proceeding on false data respecting the cause of the sounds of the heart; for the sound produced by the closing of the semilunar valves is not the second sound of the heart, but the termination of the first, and occurs in connexion with the contraction of the ventricle as the aorta pulsates, impelling the blood-wave onwards, and has no relation to the ventricular diastole we associate with it. The first sound of the heart being a prolonged sound, depends on the force with which the blood, during the contraction of the ventricle, is propelled through the aortic foramen, and recoils against the sigmoid valves as the impulse of the distended aorta is imparted to the wave; and the second sound of the heart depends on contraction of the auricles—a fact stated by Laennec, although he did not realize the principles on which it is produced.

We thus give to the aorta and its valves a more prominent place in the action of the heart than has usually been assigned to them, as we consider the aorta acts in concert with the ventricle in the propulsion of the blood-wave along the arterial system. The force of the contraction of the ventricle has been deemed sufficient to impel the blood to the extremity of the arteries. But the ventricle, in impelling the blood-wave into the arteries, distends the valves of the aorta, which, the moment the ventricular impulse is finished, reacts from its origin with increased energy, continuing the impulse on the wave, and completes the systole—the arteries communicating to the veins at each successive wave an amount of blood equal to that imparted by the ventricle. Hence it is in the propulsion of the blood along the large arteries that the stress is put upon the sigmoid valves, as they require to sustain the force of the increased reaction of the walls of the aorta.

ARTICLE VII.—*Case of Chronic Abscess treated by Carbolic Acid; with Remarks on its use in similar Cases.* By ROBERT SMITH, F.R.C.S.E.

THE patient, a man of strongly-marked strumous habit, followed no regular occupation, though, like most of the peasantry of the district in which he lived, he was compelled to work very hard at certain seasons in the cultivation of his "lot." During the early spring he had complained of unusual lassitude and fatigue, and felt himself unequal to the exertion required in its cultivation. He now noticed, too, that a swelling which had appeared on his back some time before began to enlarge. Still he felt no alarm, until, one day while at work, a severe pain "struck through his back," when, becoming faint, he went home, though it was not till a few days later that he confined himself to bed. Fourteen days after this I saw him. A bright flush then tinged his cheeks, in marked contrast to the pallor of the rest of his face, which wore a wild, frightened expression. His skin was hot and dry, his pulse small, frequent, and irregular. He was exceedingly restless; and complained of loss of appetite, with thirst; diarrhoea was present; and he suffered much discomfort from frequent and profuse perspirations at night, as well as from fitful flushings by day, followed by cold sweats. These having existed for some time, had reduced him to a state of great emaciation and debility.

On examination, a large tumour was found on the back, lying over the lower portion of the trapezius and the greater part of the latissimus dorsi muscles. It extended from the left of the mesial line, where its limits were straight and sharply defined, towards the side, and from the inferior angle of the scapula above, to the superior crest of the innominate bone below; thus embracing almost the

whole of the left side of the back. Its margins—except along the line of the supra-vertebral aponeurosis, where they were straight—were irregularly ovoid, but were accurately defined around the entire circumference by an indurated zone separating between the tumour and the sound skin beyond. There was neither discoloration nor sensible increase of heat in the part. It was painless when touched, and freely fluctuated under pressure; but from its size and position, as well as from the general irritation of its presence, it prevented the man from enjoying sound sleep at night, and thus materially tended to weaken his constitutional powers.

Its situation and connexions having been ascertained, it was opened by a moderately free incision under a pledget of lint soaked in a solution of carbolic acid, in the manner practised by Mr Annandale of this city, when a rush of thin matter of a faintly foetid odour took place; but as the patient fainted under the discharge, it became necessary to moderate the flow, and empty the abscess more gradually. As this proceeded, the smell became more offensive, and curdy matter became mixed with a semi-purulent fluid. A little over three pints of pus in all were withdrawn, after which a dressing of carbolic acid, supported by a poultice and moderate compression, was applied.

In the evening the man felt easier, and having slept during the afternoon, now seemed refreshed and cheerful. He continued thus till late at night of the next day, when he had a severe rigor. The restlessness and nervous anxiety returned; he became feverish and thirsty, and complained of heat and pain at the seat of the abscess. A dull reddish discoloration of the skin was visible over the whole surface of the tumour; the integument was dry and hot; and a thin offensive discharge was oozing through the open lips of the wound. Washing the parts with cold water, a piece of lint moistened in a strong solution of carbolic acid was introduced within the edges of the wound, and a light dressing applied over the entire surface. The parts were in a similar state on the following day, but the discharge had lost its peculiarly disagreeable smell, and had assumed the characters of healthy pus, while by the evening the integument showed a disposition to contract. Dressings of the stronger solution of the acid were now continued, and from this time the discharge gradually lessened in quantity, till, on the ninth day from the opening of the abscess, it entirely ceased, and the skin, having contracted equally over the entire surface *pari passu* with its decline, resumed the characters and appearance of health.

In addition to the local remedies indicated above, a stimulant and tonic plan of general treatment was resorted to from the first, with the occasional exhibition of a soporific. But, though this no doubt influenced the ultimate result, it only did so secondarily, conducing to the success of the case after the dangers springing from the local sore had been subdued by the local remedies applied. For the real and immediate danger to the patient in this instance lay, not in the

strumous habit, but in the risks contingent on the opening of the abscess, which was just one of those mentioned by the older writers as "chronic" or "cold" abscess, the results of opening which, if of any size, were so much dreaded by the surgeons of former days. As such it was peculiarly adapted for showing the effect of carbolic acid in this class of cases, and testing its value as a remedial agent. But we do not underrate the necessity for constitutional treatment at the same time; for, even in the case we are considering, the scrofulous habit of the patient formed a complication which it would have been imprudent to have overlooked in practice. It was even the remote cause for the appearance of the tumour at all, and certainly aggravated the dangers of its presence. But the risk from this source was remote, and would have been less active in its operation if left to itself than that which actually sprang up, or indeed than any of the risks which might have ensued on the opening of the tumour. It would have destroyed life by the comparatively slow process of exhausting the powers of the system after a more or less protracted period of hectic irritation had been endured by the sufferer; and thus, for the time, it became a secondary, though still an important element in the consideration of the line of treatment to be pursued. What claimed the most serious attention, from the moment the tumour was opened, were the dangers that might follow the evacuation of such a large collection of pus, to the presence of which the body had become accustomed, and therefore tolerant while it remained undisturbed. And so well was this understood by the earlier practitioners, that various modes of emptying such an abscess were proposed, with more or less reference to the general principle of lessening the shock to the system by gradually accustoming it to the change, or intended to obviate the dangers too surely foreseen from the free entrance of impure air within the sac of the abscess, which so often proved fatal in the experience of the older surgeons in similar circumstances.

It is in these circumstances that the good effects of carbolic acid in treatment become apparent, though its precise mode of action is still undetermined, or at least has not yet been fully and scientifically explained. It has been said that it operates by destroying certain organic germs existing in atmospheric air. But in the absence of any demonstration of the positive existence of such germs, any reasoning founded on their assumed existence can be only hypothetical and inconclusive. Yet, in ascribing the therapeutical effects of the acid generally to its antiseptic qualities, this theory undoubtedly points to the true reason of its medicinal value. It is at once an antiseptic and a disinfectant—perhaps the best disinfectant we possess—and thus it has a twofold action. Its antiseptic properties are of the utmost value when, from changes having their origin within the system, or from external conditions impressed on the sore, an altered or perverted local action has resulted, and the part has assumed an unhealthy aspect; and it is useful generally as a disin-

fectant by destroying or removing impurities existing in the atmosphere, which might otherwise become absorbed were free access to a granulating surface permitted, and so lead to serious local changes, which acting on a shattered constitution might only be the precursors of death. And since it is impossible, were it desirable, to occlude atmospheric air entirely from the surface of a wound, it becomes of the first importance to free it from substances known to have a deleterious action on such a surface, permitting only such as is pure to come in contact with it. For pure air seems to have no injurious effect on a raw surface, though any impurity in it does harm. Hence the necessity for disinfecting—that is, destroying or transforming any such substance by presenting a body with which it may enter into new and harmless combinations.

In addition to either of its actions as an antiseptic or as a disinfectant, carbolic acid has a third, which is productive of great benefit in its application to the treatment of granulating surfaces. In several cases in which I have used it, it has appeared to attack any superfluous moisture about the sore, and removing the oedema which presents such an obstacle to recovery, has left the part in a condition favourable to healthy action. The swollen and thickened edges have disappeared, and the granulations, previously large, pale, and oedematous, have become small, and possessed of the characters of healthy granulation. Now, the acid has a powerful affinity for water; and an experiment on the healthy skin strengthens this view of its *modus operandi*. A strong solution of the acid placed on a portion of healthy integument, if allowed to remain, acts as a caustic. If washed off in a short time, the skin on which it rested becomes dry and contracted in the course of six hours,—a condition which is only removed in the course of five or six days, during which period a slow process of desquamation has been taking place. A less powerful solution applied in the same way has a similar effect. Thus, it appears to have a property of absorbing or removing moisture, while it exercises an astringent effect on the parts, both of which qualities in proper circumstances might be of the utmost value in the treatment of wounds or granulating sores.

ARTICLE VIII.—*On the Cause of the Prophylactic and Therapeutic Value of Table Salt, Iodide of Potassium, etc.* By Dr JOHN G. MACVICAR, Moffat, N.B.

WHEN table salt is so cheap and so easily procurable by everybody as it is in this country, one is apt to think but little as to its real value. The general impression is, that it is added to food merely to improve its taste. What happens, however, when it cannot be had, either as a natural constituent or an addition to our food, proves that the ingestion of salt is essential to health. But why it

should be so, science gives no account. No doubt, there is reason to believe that muriatic acid, which table salt alone can supply, is useful if not actually needed for digestion; but it may be argued that, in case of its absence, lactic acid may take its place and do its work.¹ And, altogether, it must be admitted, that table salt always has possessed an eminence among saline substances which the actual state of physiological chemistry does not explain.

Having accomplished a molecular construction according to the method which I pursue—which, during its progress, keeps me as much in the dark as to its issue as the discussion of an algebraic equation before it is completed, keeps the algebraist in the dark as to its ultimate form—an extraordinary relation presented itself between the structure of the atom of table salt and that ultimate excretion from the blood, on the due construction of which, and its elimination from the organism, it is well known that health depends—in a word, the atom of urea.

To those who, in keeping with the chemical hypothesis at present most popular, look upon chlorine and sodium as simple atoms, essentially dissimilar to each other, and unchangeable into anything else through all time, the suggestion of a structural relation between them, and still more between the product of their union, chloride of sodium and urea, will seem simply absurd. But even to those who hold this view, and who refuse to take into consideration any other conception as to the intimate nature of the chemical atoms, but that each is a little solid *sui generis*, something that is not without interest may be shown in the relation of sea salt and urea.

Thus, as to their atomic weights, if we take, as was long done (and as should be done in certain cases still), the atomic weight of chlorine at 36 and of sodium (when in sea salt) at 24, instead of 23 which it is when existing free, then the atomic weight of the atom of sea salt and the atom of urea are the same.

$$\begin{array}{lcl} \text{Sea salt,} & \text{ClNa} = & 36 + 24 \\ \text{Urea,} & \text{C}_2\text{N}_2\text{H}_4\text{O}_2 = & 12 + 28 + 4 + 16 \end{array} \} = 60.$$

Nor is this the only relation which presents itself at once. Thus, these substances are given to nature in what may be regarded as her first and her last organically composed liquids—namely, the water which fills the bed of the ocean on the one hand, and that which fills the bed of the vessel prepared for its reception in the organism of the highest animals on the other. Nay, keeping in mind that, since chloride of sodium is a binary combination, a single atom cannot possibly be symmetrical, and that to constitute a symmetrical or insulable structure a couple of atoms at least is required, while a single atom of urea, consisting as it does wholly of coupled

¹ In the molecular morphology which I advocate, chlorine, and what, by analogy, might be called lactine, $= \frac{1}{2} (\text{C}_2\text{O}_2)$, are isomorphous, and very nearly isobaric. As to the movable atoms of H and of O, associated with it in the living organism, chemistry can neither discover nor verify them.

elements, is symmetrical or insulable by itself, we obtain, as the investiture of the least insulable portions of sea salt in the waters of the ocean, and of urea in urine, the same quantity of water. Each appears to exist as a nucleus in the centre of the same aqueous cell or capsule of aqueous matter. According to this view, the normal percentage of urea in urine ought to be just half that of table salt in sea water. Now, this appears to be ordinarily the case. Thus we have

Table salt in sea water,	2.53 per cent.
Urea in urine,	1.26 „

But, considering the all-embracing harmony and marvellous assimilative power of nature, may we not expect that occasionally, though not necessarily or normally, there may be as a nucleus in the aqueous capsule in urine a coupled atom of urea, as there is of table salt in sea water? This would give 2.53 per cent. of urea. Now, in point of fact, it appears that, within the conditions of health, when an unusual quantity of effete matter requires to be eliminated from the living system, the quantity of urea in relation to aqueous matter may be doubled without injury.

I have elsewhere shown¹ that the quantity of aqueous matter referred to possesses molecular unity, and is what, when in the atmosphere, may be called a particle of drizzle, meaning by this term the smallest quantity of water which tends to individualize itself in nature. And the inference from the relation between urea and water in urine which has now been stated, if it be accepted, is this, that supposing there were nothing else in urine but urea, the relation which the physician should endeavour to establish as the condition of perfect health, in so far as this excretion and the protection of the membranes with which it comes in contact are concerned, is 12.65 of urea per 1000 of urine. It is not to be forgotten, however, that urea is only one of many non-aqueous substances which enter into this highly composite liquid; the discovery of whose normal constitution, in reference to all its constituents, must be extremely difficult, and will be possible only to a much more advanced state of science than that which now exists. This much, then, for those who look upon the chemical elements as simple.

But there are others, and the exigencies of science are constantly increasing their numbers, who apprehend that the undecomposable particles of the chemist cannot be the ultimate atoms or elements of matter. And now that a reaction against the absurdities of alchemy is no longer called for, the thought may certainly be reasonably entertained, that possibly the chemical atoms, like all other individualized particles of matter, possess a structure that is molecular, though too stable to be decomposed in the present state of chemical analysis, and that, though not in the course of the life

¹ See *A Sketch of a Philosophy*, Part III., p. 68, "The Chemistry of Natural Substances." Williams and Norgate: 1870.

of man, yet, secularly, in the course of the life of nature, certain of them may, by dedoubling, by giving off occluded hydrogen, or otherwise, be transmutable into certain others.

It was when working on this conception, and when I had satisfied myself as to the intimate structure of the atom of chloride of sodium, and compared it with that of urea, that the following function presented itself. *Chloride of sodium, when working under the secular action of its own intrinsic heat towards a more perfect definition and articulation of its elemental particles, must, in so doing, be also working towards the construction of urea, and thus assisting in defecating the blood, etc. And what a chloride could do in this way, a bromide, and still more an iodide, could do more effectually, though not so safely, since all the halogens are isomorphs, and function similarly, but with a force proportional to their atomic weights.* Although, during the longest life of any individual, never an atom of table salt were decomposed otherwise than into chlorine and sodium, still, acting in the direction of urea, it must affect catalytically or assimilatively the material suited for the construction of urea that lies around it in the living organism, and thus be of great value in assisting to construct urea, and so in putting the effete matter in the system in a state in which, instead of poisoning the blood and tearing up the epithelium wherever it comes in contact with it, it can, as a particle of urine, be passed on and eliminated, safely enclosed in a capsule of water, as has been shown.

Perhaps these views, though new to most persons, may yet seem so reasonable as to induce some readers to wish to hear a little more upon the subject. I therefore proceed; for it seems to me that, within the compass of a very few pages, I could give an idea of the molecular structure of the atoms both of table salt and of urea, so as to show the relation which subsists between them, according to the method of molecular morphology which I advocate.

But here, for the sake of having graphic formulæ, it will be desirable to have recourse to the same symbols as were introduced into this Journal before, premising only that when $C = 12$ and $O = 16$, as is most usually done in this country at present, we view these as doubled atoms, mistaken for single ones (on the same reasoning as would lead to the inference, if the eyes of vertebrate animals happened to be like the atoms of bodies so small as to be invisible, that all these animals had one eye only, inasmuch as both their eyes normally move together, and give single vision as their normal function). Thus, for graphic symbols of hydrogen and oxygen, we take the ciphers which express their atomic weight:

| for an atom of hydrogen.

8 or ∞ for an atom of oxygen.

① for an atom of azote.

This constituent of the atmosphere comes out in our system, like its

sister constituent, oxygen gas, as a coupled atom. Its half, which we usually call zote (its graphic symbol a pharmaceutical mark), being intensely dissymmetrical, cannot exist singly in the free state. But it is that which invests the nitric bodies with their intense activity. And while it is yet an abyssal, and not an atmospheric element, it plays perhaps the most important part in nature, being half silicon in the abyss, as it is half azote in the atmosphere, and, generally speaking, none other than an element of lithium; its lustre being metallic when its expanded or cleft pole is peripherad in the molecule which it constitutes, as in the case of lithium; and only metalloidal when its lens-shaped pole is peripherad, as in the case of azote and silicon. No doubt, such statements thus crowded upon each other, and all of them new, save to the few who have made a study of molecular morphology as we advocate it, will seem strange and unwarrantable. But to stop here to point out those features in a molecular structure which imparts to it a metallic, or, on the contrary, only a metalloidal or a non-metallic structure, as the case may be, as also to show how molecular structures, whose nuclei (in the midst of their ætherial atmospheres or dynamispheres) are the same, should yet be found in very different places in nature, and possess very different relations to the aeriform, as lithium (= half silicon) and zote (= half azote), would detain us too long. Perhaps the instructed reader will let this difficulty pass here, and if he finds reasonable speculation in the rest of the paper, he will credit this part with the same advantage. Let us then proceed.

When lithium (lithhium) contains all the matter involved in its genesis, its atomic weight is not 7 but 8. It contains an atom of occluded hydrogen, which mars its symmetry, and which it tends therefore to give off. In this state we may use the scruple symbol, with the central point of the trident thickened, thus—

☞.....crude lithium, atomic weight = 8.

Moreover, after it has given out this atom of hydrogen, and attained the weight of 7, its polar regions are still unlike each other. To assimilate the metallic, cleft or expanded pole, to the metalloidal, closed or lens-shaped pole, another atom of hydrogen must be given off; for hydrogen in our system is not only a metallic element, but it is the very essence or principle of metallicity, that is, a molecule is metallic when its periphery is studded with semi-occluded inseparable hydrogen. When, on the contrary, that hydrogen has secularly worked itself out and been given off, then the same molecule (its atomic weight lightened by the weight of the hydrogen which has left it) is no longer metallic; it is metalloidal or non-metallic.

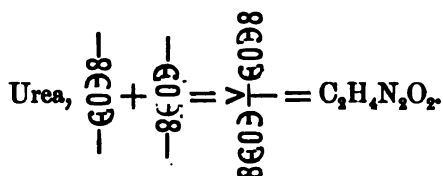
Thus, when two atoms of hydrogen have been evolved out of one atom of crude lithium, our elemental structure has acquired symmetry. It is now a lens-shaped form, having its atomic weight $8 - 2 = 6$. And this is found to represent in every feature and

stituted by three atoms of carbon. But the atom of hydrogen, when loaded by carbon to this extent, is no longer capable of assuming the aeriform state, and therefore remains unknown to the chemist as a free substance.

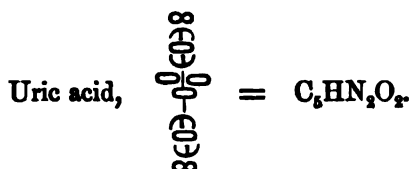
Urea.—But along with the atom of carbon on each of the poles of an atom of marsh gas, these poles may carry many other elements. And when, capping or mailing each pole of such structures, there is an atom of oxygen, so as to warn off that parasite which intrudes itself everywhere, then such molecular structures may possibly possess no small stability. And here, let us suppose that, just as in *expiration* the atom of effete carbon is caged in between the two parts of a unit of oxygen gas, and is in this way carried out of the organism into the atmosphere, so in *urination* the atom of effete carbon is caged in between the two parts of a unit of azotic gas, giving as homologous

In the lungs $\begin{array}{c} \text{O} \\ \text{O} \end{array}$ and $\begin{array}{c} \text{O} \\ \text{O} \end{array}$ in the kidneys.
Carbonic dioxide. Carbonic dizote.

Then supplying the latter with oxygen, as HO on one pole and with an atom of H on the other pole (which, under the law of red-integration, both the atom of carbon and of zote must tend to secure), and then doubling, as in the genesis of marsh gas itself, and all similar structures when hydrogen is the terminal of both poles, we obtain



In certain orders of animals, however, almost entirely, and in the highest animals to a certain extent, the three equatorial hydrogens are liable to be substituted by three carbons. And in this case there results dry monobasic.



And here a most interesting product of the laboratory offers itself for our construction, which is obvious, namely, chloral; as also, indeed, many others. But let us lose no time in coming, if possible, to understand the structure of the atom of table salt on the same

principles. This implies a knowledge of sodium and of chlorine, which latter, as we shall presently see, implies also a knowledge of potassium.

Sodium.—It has been stated that our dissymmetrical or heteropolar element, whose atomic weight, when crude or unreduced, is 8, and when reduced is 7, when in a molecule with its expanded pole peripherad represents an atom of lithium. Let there then be a suboxide of lithium, in which the two sorts of lithium, the crude and the reduced, are, as we may say, rusted both of them into an atom of occluded oxygen as the medial coupling joint, and we shall have a metallic structure which we may well regard as undecomposable in the laboratory. Its atomic weight also is such that we may at once hold it to represent—

$$\text{Suboxide of lithium, } = \overset{\text{At. weight.}}{\infty} = 7 + 8 + 8 = 23 = \text{Sodium.}$$

If both atoms of lithium were in the crude or unreduced state, a repulsion would exist between them in virtue of their identity. Moreover, as the atom of oxygen itself is capable of dedoubling into two semi-metallic elements (atoms of beryllium), and as both halves of the atom of sodium would be symmetrical in reference each to its own centre as well as to the whole and each half, it is only to be expected in this case that the atom of sodium, if free, would dedouble under heat or any adequate analytic ordeal into two metallic elements analogous to itself, but possessing half the atomic weight of this full-weighted sodium, that is, $\frac{1}{2} = 12$. Hence the equation :

$$\text{One atom sodium} = \text{two atoms magnesium.}$$

The letter h introduced into the word sodhium implying that the sodium which can thus dedouble into magnesium contains an occluded atom of hydrogen, which has been given off from that sodium, which can exist free and is permanent in nature; its atomic weight = 23.

And here we may remark, that plainly there may also be a fully reduced sodium, in which both the lithiums have been reduced, and its atomic weight consequently $7 + 8 + 7 = 22$. And hence an inadequacy of an average of experiments with the balance to give true atomic weights in such a case; for the mean of 24 and 22 is 23.

Potassium, Kalium.—Considering the tendency of dissimilars in general to unite together, it may be expected that where a highly synthetic mode of action exists, an atom of that sodium, which consists of two identical polar parts, and which is comparatively easily decomposable, shall unite with and merge into an atom of the indecomposable sort, while the atom of oxygen, entering into the former and now set free, will attach itself to one pole of this now double-flowering sodium. Neglecting the oxygen for the present,

we thus obtain a metal analogous to sodium, while it in every way represents—

$$\text{Hyposuboxide of } \left. \begin{array}{l} \text{lithium,} \\ \text{ægeæ} \end{array} \right\} \text{ægeæ} = 8 + 7 + 8 + 8 + 8 = 39 = \text{Potassium.}$$

At. weight.

Such a structure may also be effected when all the atoms of lithium are unreduced, in which case its atomic weight would of course be higher by one atom of hydrogen, and therefore = 40. But in this case, just as in that of that sodium whose atomic weight was 24, each half is symmetrical both in reference to the whole and to its own centre. As before, therefore, and indeed more readily here, in consequence of the greater length of axis, dedoublement may be expected, and two atoms given to nature of a new metal analogous to magnesium, but its atomic weight $\frac{1}{2} = 20$. In a word, we obtain the following equation :

One atom of kalhium = two atoms of calcium (kalkium).

Chlorine.—Ultimately, however—as, for instance, by being secularly in the aeriform state under the ordeal of adequate heat—both atoms of lithium in the atom of sodium must be reduced, so that its atomic weight shall be only 22. And since both lithiums now entering into its structure are dissimilar to crude lithiums, the latter will all the more readily rush in upon them and unite with them and firmly cohere. We shall thus have a potassium whose atomic weight is only 38. (Hence there is the same uncertainty as before when the balance gives us as the average of weighings 39.)

But this potassium, whose weight is 38, cannot dedouble like that whose weight is 40; for each half is not symmetrical on both sides of its own centre. Supposing it then to be secularly exposed to an analyzing ordeal, such as heat, while it is in the free or aeriform state, in what way will its axis be shortened so as to bring it more into keeping with the law of sphericity, which may be shown to be that in which the law of symmetry or of statical equilibrium culminates? To this the answer is, that the atom of hydrogen, half occluded in each pole, expanding and imparting a metallic character to both poles, will be thrown out and given off. Now, by this emission, both poles will become lens-shaped and carbon-like. The whole will be metalloidal, and it is such that it represents an atom of full-weighted

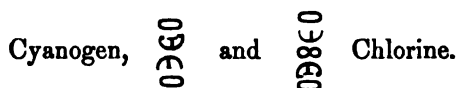
$$\text{Kali-demetalized, } \left. \begin{array}{l} \text{ægeæ} \\ \text{ægeæ} \end{array} \right\} \text{ægeæ} \text{ At. w.} = 7 + 7 + 8 + 7 + 7 = 36 = \left\{ \begin{array}{l} \text{Chlorine.} \\ \text{Klorine.} \end{array} \right.$$

Nor will it be decomposable any more than the atom of light sodium which constitutes its interior; for the polar bodies are not insulable by themselves. If the atomic weight of each of these had been the even number 6, they would have been insulable by

themselves and separable. They would, in fact, in that case have been atoms of carbon. But their atomic weight is the odd number 7, which implies dissymmetry of structure and incapacity for the free state. Still, while *in situ* on the poles of the atom of chlorine, they must function generally as carbons, and this explains much of the habits of chlorine.

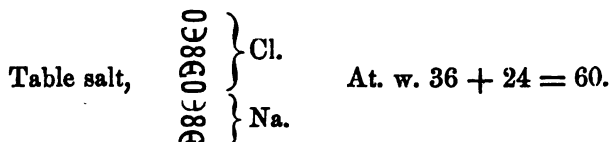
As to chlorine, it is not to be expected in nature in the free state, since during its genesis it has given off from first to last no fewer than 4 atoms of hydrogen. Notwithstanding the great length of its axis, therefore, it may certainly be expected, under the law of redintegration, to detain or recover one atom of hydrogen; for it contains occluded within it an atom of oxygen, and as a magnet, though concealed, draws a needle, so will this atom of oxygen in chlorine attach one atom of hydrogen to it, so as to redintegrate the primeval HO. And so with the other halogens whose structure is the same as that of chlorine.

The approach in structure of cyanogen to chlorine will here be marked by the instructed reader:



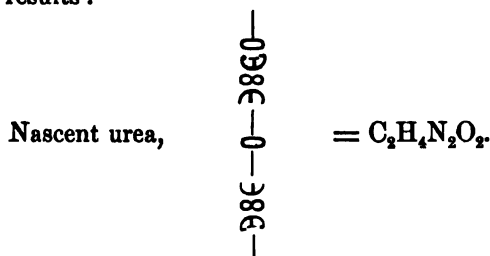
The main difference lies in the atom of oxygen occluded in the atom of chlorine. But this difference of structure, though small, implies a complete difference in physiological functioning, for it secures to chlorine extreme stability and permanency. Cyanogen, on the contrary, having no coupling bond for its two parts, must be very versatile. As a hydroacid, indeed, which is its natural state, it is capable of assuming no fewer than five different forms. Now, such versatility seems to imply poison. But that by the way.

Table Salt.—Supposing potassium and sodium both to exist under an ordeal, as, for instance, under heat, in the aeriform state, then sodium, in consequence of its comparatively short axis and small departure from the law of sphericity, will continue to exist unchanged and in the metallic or wholly unreduced state, while potassium, for the opposite reason, will be reduced, and from metallic will have become metalloidal. Or, as we may say, it will be changed from K to Kl (which, according to these views, would be a better literal symbol than Cl). But the two, Na and Cl, being now eminently dissimilar, will rush together and unite, and thus there will result an atom of full-weighted

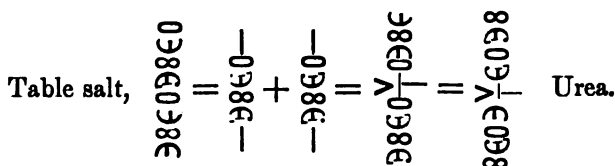


Now, on inspecting this structure minutely, it will be seen that

within the axis there are four atoms of hydrogen occluded. And these, during the process of symmetrizing the axis by secular or vital action, must be ever tending to develop themselves. This done, there results :



Now, of this the axis is much too long for stability, and the various constituent elements are not symmetrically disposed. But there is here the material for an atom of marsh gas, with a beautiful oxycarbazotic structure on each pole. First, by dedoubling, therefore, and assimilating the two parts; then (as so often in nature) by the construction of a tetratom of hydrogen as the equatorial body, we may have the equation :



It is not to this transformation, however, considered as actually occurring, that the attention of the reader is directed, but to the secular nixus towards it in table salt ingested, which, mirroring itself in the loose effete products of the degeneration of tissue, tends to assimilate them, as is both easy and natural, to what itself is aiming at, that is, to assist them in constructing urea, and so in defecating the blood, the humours, the system generally.

But whatever the value of the chloride for this purpose, the value of the corresponding iodide must be far greater, unless, indeed, it be too powerful to be taken into the stomach at all; for iodine, according to our views, is a barytic isomorph of chlorine functioning similarly, but vastly more powerfully, while, being also isovoluminous, it is no less penetrative. What the corresponding iodide is, I do not know. Intermediate in power between the chloride and the iodide is the bromide.

ARTICLE IX.—*Case of Recovery from Extensive Wound of the Brain.* By Dr M'LEOD, Hawick.

THE following is a good instance of the amount of injury that may be sustained by the brain and system generally without a fatal result.

On 8th July 1867, to avoid an imminent collision, the driver and stoker of a train on the Waverley line jumped off their engine while going at a speed estimated at fifty miles an hour; the stoker was comparatively little injured, but the driver was picked up for dead, and for some time showed but very slight signs of animation.

I was on the spot half an hour after the accident, and found him labouring under violent convulsions, bleeding freely from the nose, mouth, and ears, and very copiously from a transverse wound of the left eyelid, from which blood welled in large quantities as each convulsion fit grew stronger. From this wound a portion of brain protruded, and on passing the finger into it I found the upper part of the orbit completely smashed, and an opening in the skull sufficiently large to admit my finger easily. Having removed one or two splinters of bone (all I could find) and the protruding portion of brain, which was much broken, I stitched up the wound, leaving an opening for the escape of blood, which was still flowing, though in much less quantity. This was about 4 P.M., and he continued much in the same state till about 6, when he vomited nearly a handbasinful of dark blood. The convulsions had by this time ceased, and he remained in a state of coma. On being undressed he was found to have sustained severe bruises on the chest, sides, and back, but no broken bones in this region. Expecting he would certainly die before morning, I left, with instructions to his attendants to keep the house cool and quiet, and apply water-dressings to his wounds. Next morning I found him slightly improved, pulse 80, and full. He had passed a restless night, and had again vomited some blood. He continued much in the same state till the 10th, when he began to talk somewhat coherently, and recognise those around him. The part of the wound which was stitched was healing kindly, but through the opening there came a copious dark discharge. On the 13th he was quite sensible. 15th, condition much improved. 27th, left for his home at Newcastle, much against my will, but stood the journey well; and I have heard from his medical attendant there, that he progressed rapidly towards recovery.

The wound of the orbit must have been caused by his coming against some sharp projection of a waggon which he struck on jumping; and the quantity of brain lost would weigh, as nearly as I could guess, above a quarter of an ounce.

ARTICLE X.—*Method of Examining and Recording Medical Cases.*

By WILLIAM R. SANDERS, M.D., etc., Professor of Pathology and of Clinical Medicine in the University of Edinburgh.

AFTER a good many years' experience of clinical work, I have thought it might not be without interest to explain the general method which I have adopted in investigating medical cases, especially in connexion with the teaching of clinical medicine. It is not on the score of novelty, but of usefulness, that I would venture to recommend the procedure I am going to describe, or some modification of it. Though comprehending many details, the plan is simple in its principle, and, at the bedside, I have found it work satisfactorily. It is intended chiefly for students, but I have had evidence to convince me that some such method would be found of service to practitioners, as an aid towards that accurate observation of medical cases which is a leading feature, as it is an undoubted necessity, of the medicine of the present day.

Many methods of examining patients have been proposed, but hitherto none has been universally accepted. Perhaps variety is necessary to suit different minds, or particular circumstances. Generally speaking, however, the plans suggested have been mainly founded either on an anatomical division of the body into Regions, or on a physiological division according to Systems, as the circulatory, respiratory, nervous, etc. The arrangement according to Systems was devised in 1814–1826, and tabulated by M. Rostan, who seems first to have perceived the importance of definite method in clinical work. Rostan's plan was introduced into our Royal Infirmary by Professor Bennett many years ago, and was always employed in his course of clinical instruction. The method which I purpose now to describe, consists mainly of a combination of the regional and the physiological plans of arrangement. For clinical inspection, the body is divided into certain Regions, and the Systems of organs are taken in connexion with the region to which they respectively belong. The successive examination of the various regions, with their included systems, ensures a thorough investigation of the whole organism. A complete account is thus obtained of the "present condition," the "status presens," of the patient, which is the main object, the backbone of any method of case-taking. The other parts of the investigation, viz., the antecedent history of the patient, and the diagnosis, prognosis, treatment, etc., are arranged in the general scheme in appropriate positions before or after the Regional examination.

The directions for case-taking according to the method now proposed have been drawn up in a tabular form or scheme. The items given under the different headings are intended as examples or hints of the kind of information wanted; they are suggestive, not exhaustive; sufficient for ordinary purposes so far as space

would allow. In former years this table was written on a large sheet of paper, and suspended in the side-room of the wards; but lately the plan has been adopted of printing the scheme on cards of convenient size, which are distributed to students of the clinical class. In using this scheme, it is to be remembered that the order of examining the case is the same as the order of recording or reporting it. To some it might seem more convenient to examine in one way, and afterwards adjust the report differently in some more precise form; but the advantages of using the same mode of examination and reporting are so decided, that I think a uniform plan should always be adopted.

I shall now submit the general Scheme or "Method of Case-taking," and shall afterwards subjoin such explanations and remarks as appear necessary.

METHOD OF CASE-TAKING.

Disease _____
Result _____

I. Preliminary Information.

1. Name, Age, Occupation, Residence, Date of Admission, Number of Ward and Bed.
2. Complaints, (*a.*) as stated by patient, or (*b.*) reported by friends, also (*c.*) obvious morbid appearances, *e.g.*, jaundice.
3. History (*a.*) of present attack, (*b.*) of previous health, (*c.*) history of social condition and habits, (*d.*) family history.

II. Present Condition—Regional Examination.

REGIONS A. EXTERNAL.

1. General condition as to conformation or development, height, weight, muscularity, whether confined to bed, posture, etc.
2. Aspect and expression of face.
3. Integument (*i.e.*, skin and subcutaneous texture, including glands), as to temperature, perspiration, eruptions, tumours, etc.
4. Condition of limbs and joints.

REGIONS B. INTERNAL.

N.B.—Here commence with the Region and System presumably most diseased, also describe all the systems of one region before going to another.

HEAD.*NERVOUS SYSTEM.*

- (a.) *Peripheral*.—Nerves, as to motion, sensation, special senses (ophthalmoscope).
- (b.) *Central*.—Brain and spinal cord, as to feeling of well-being or exhaustion, intelligence, sleep, headache, vertigo, nausea, etc.

THORAX.*RESPIRATORY SYSTEM.*

- 1st, *Symptoms*.—Number of respirations; dyspnoea, pain, cough, amount and character of expectoration, etc.
- 2d, *Physical Examination*.—Chiefly by inspection, palpation, percussion, auscultation of organs—
 - (a.) *Peripheral*.—Nose, action of alæ nasi, larynx, voice (laryngoscope), trachea, etc.
 - (b.) *Central*.—Lungs and pleura.

CIRCULATORY SYSTEM.

- 1st, *Symptoms*.—Cardiac dyspnoea, palpitation, pain at præcordia, syncope, angina pectoris, etc.
 - 2d, *Physical examination* by inspection, palpation, percussion, auscultation of organs—
 - (a.) *Peripheral*.—Arteries, especially radial and carotid; pulse as to number, character, variations, etc. Veins.
 - (b.) *Central*.—Heart and large vessels within the thorax.
- § State of the blood; microscopic examination, etc.
§ Intra-thoracic tumours.

N.B.—The lymphatics and lymphatic glands are examined locally in the regions in which they occur.

ABDOMEN.*DIGESTIVE SYSTEM.*

Examination as to Symptoms and Physical signs of—

- (a.) *Intestinal Tube*.—Teeth, tongue; hunger, thirst; deglutition, digestion, vomiting, stools, etc.
- (b.) *Walls and Solid Viscera of Abdomen*.—Shape, swelling, etc.; condition of peritoneum; liver; spleen; pancreas; lymphatic glands; tumours, etc.

GENITO-URINARY SYSTEM.

Frequency of micturition; pain; difficulty.

Urine, amount in twenty-four hours; specific gravity; colour and transparency; acidity; odour; deposits; chemical and microscopical examination.

Examination of kidney, bladder, urethra, etc.

Menstruation; condition of vagina and uterus, etc.; pregnancy, etc.; tumours.

III. Diagnosis.

Nature of Disease, as founded on brief summary of the functional disorders and structural lesions made out. These are arranged in the order of their importance or probable succession.

IV. Prognosis.

Immediate and remote.

V. Treatment.

Diet and regimen. Principles and special indications. Copy of prescriptions.

VI. Progress of Case.

Reports daily, or as occasion requires. Condition ascertained according to the order of Regions and Systems above given. Any changes or new phenomena specially noted.

VII. Termination of the Case.

Date and nature of the result. If necessary, patient's address in town or country noted. If fatal, and a post-mortem held, enter the report.

Explanatory Remarks.—It will be observed that, at the head of the case, blank spaces are left for the name of the disease and the result of the case. These, which are to serve as Title, should not be filled in till the case is completed. The case-taking proper is divided into seven sections:—Preliminary Information, Present Condition, Diagnosis, Prognosis, Treatment, and, finally, Progress and Termination of the case. On each of these, but especially on the first and second, I have some remarks to make. Before doing so, however, I should mention a necessary precaution. Either before a case is formally taken, or, at all events, during the initial inquiries, the observer should satisfy himself whether the case is evidently, 1st, Febrile or non-febrile (as shown by heat of skin and rapid pulse); 2d, Whether the patient is intelligent and able to answer questions; 3d, Whether he is in a condition to undergo the fatigue of physical examination. If the patient is apparently affected with fever, or is delirious (I have known a patient in delirium tremens subjected uselessly to a painstaking interrogatory according to a fixed method), or if he is in a weak or dangerous condition, those circumstances should be reported, and the course of examination modified accordingly. To this precaution the physician or his assistant usually attends, but it is well that beginners should also be aware of it.

I. *Preliminary Information*, which is the first division, comprises:—

1st, The designation, *i.e.*, name, age, occupation, etc., date of admission (*giving day of month and year*), etc.

2d, Complaints of patient: *i.e.*, a brief statement of all his alleged ailments. This information is usually derived from the patient himself. If his complaints are definite, *e.g.*, "complaints of cough," "looseness of the bowels," etc., these are set down simply: but if his symptoms are vague, we must say so, and endeavour to ascertain cursorily, whether his ailments are general or local, as indicated by pain, etc. Sometimes it is told us on the part of friends, previous medical attendant, or by the patient himself, that his case is one of "heart disease," "diabetes," etc. When thus obtained, the information should be entered upon as "Reported (by—) to be suffering from," etc. Occasionally a patient is admitted, who is seen at once to be affected with "jaundice," etc., which fact may at once be set down, along with his other complaints. Lastly, patients may be brought in comatose,—or in a fit, or delirious, etc. Unusual circumstances like these, require of course modifications in the statement, which are suggested by the events themselves. All that is wanted at this stage, is a first indication as to the probable nature of the case, so as to guide the further examination, on which alone, and not on first impressions, the diagnosis must be founded.

3d, Antecedent History (Anamnesis). This includes, first and chiefly, the account of the present attack, its presumed cause, date of commencement, first symptoms, and subsequent course, till the time of admission. Next, the previous state of health, and especially former diseases, are noted. Inquiry may then be made as to the social condition and habits (clothing, lodging, food, intemperance, syphilis, mercury, etc.); and, lastly, any important particulars of family history, as to hereditary diseases and general health or longevity, may be taken down.

In regard to etiology, diagnosis, and prognosis, the history or anamnesis is of great importance. But it is a very difficult part of case-taking, owing to the want of recollection or the deficient powers of observation or description on the part of the patient. Here patience, and especially tact in eliciting reliable facts and dates, with succinctness in narration, are qualities which the observer ought to cultivate.

In the course of careful case-taking, a doubt may sometimes arise, whether the antecedent history should be taken at this stage of the inquiry, or had better be postponed till after the Regional examination has been made. Much may be said in favour of either view. On the whole, however, after experience of both ways, I think that, when the case is chronic, and time permits, it is best to obtain the history at once, before proceeding to the investigation of regions and systems. But if the patient is very weak, or in cases of acute disease (*e.g.*, pneumonia), or when the information must be got from friends, etc., the history may be

meanwhile omitted, or obtained in part now, and left to be completed afterwards.

II. *Present Condition—Regional Examination.*—This constitutes the chief part of the case. Clinical experience has proved, that it is not enough to examine merely the parts or organs which are presumed to be diseased. These should be more specially investigated, but at the same time a general review of the whole organism must also be made. With method and practice this can be done quickly; but by beginners it should always be done carefully. Otherwise, reliable results cannot be obtained, and serious errors and oversights are certain to occur.

As previously stated, the examination is to be conducted according to Regions, the physiological systems being taken in connexion with the regions in which their chief organs lie. The division of the body into regions, when once understood, of itself settles the different subdivisions of the inquiry; no effort of memory is needed to discover what system comes next, or whether all have been examined or not. The whole body is gone over in regular order, and when that is finished, the examination is necessarily complete.

For clinical purposes, the body may be divided into A, the External Regions, viz., the external or integumentary surface, and the limbs; and B, the Internal Regions, viz., the three great visceral cavities, head (cranio-vertebral cavity)—thorax—abdomen.

In the Regional examination, it appears to me essential that the External regions should always be taken *first*. 1st, because they are most accessible; 2d, because the information they supply is immediately useful—(e.g., temperature of skin as indication of pyrexia or apyrexia; eruptions, dropsy, etc.); 3d, because apt to be forgotten if left to the last. This external examination may be done very rapidly, except in cases, e.g., of skin eruption, or acute rheumatism, in which the external regions are often the most important. The external regions should be taken in the order indicated by the numbers 1, 2, 3, 4.

When the External examination is completed, we next proceed to the three Internal cavities, for the purpose of observing the state of the walls of these cavities; their contained organs; and, in connexion with these organs, the physiological systems of which they are the centres. In relation to the Head, we have only one system to investigate—viz., the nervous system. The Thorax comprehends two systems—the respiratory and the circulatory; while the Abdomen includes three systems—viz., the digestive, the urinary, and the generative. It is made a rule that, in the course of the examination, the systems contained in the same cavity should always be taken immediately one after the other, before we pass to the systems in another cavity. Thus, the respiratory and circulatory systems being both thoracic, must be examined in immediate sequence; and, in the abdominal region, the digestive,

urinary, and generative systems must be kept in close relation to each other. This rule does not forbid that in the thoracic region the circulatory may, if thought fit, be taken before the respiratory system, or *vice versâ*; or that in the abdominal region the genito-urinary system may take precedence and be followed by the digestive or *vice versâ*. But it requires that the systems belonging to different regions shall not be intermixed. We must not go at random from the urinary to the respiratory, and from that to the nervous system, next to the digestive, and finally to the circulatory system, etc. To do so would risk confusion and omissions. It would violate the principle of Regional division, the advantage of which consists partly in associating together those systems whose chief organs are in anatomical proximity, and partly in securing that when a region has been examined, the included systems have necessarily been examined with it.

On commencing the investigation of the Internal regions or cavities, and their relative systems, the question occurs, Which region should be first examined? and which system in that region should have precedence? In the Table, the head happens to be placed first, then the thorax, and lastly the abdomen. But no necessary precedence is intended by this order. On the contrary, as it is a matter of indifference, *per se*, which internal region is taken first, the rule is given to begin with that region and system which the previous inquiries have indicated as the chief seat of disease. Thus, in cases of paralysis, we should, after finishing the external regions, begin the internal examination with the nervous system (it would be absurd to begin with the digestive organs); in heart disease, with the circulatory organs; in phthisis pulmonalis, with the respiratory system, etc. After finishing the systems in one internal region or cavity, we take next the cavity which has most connexion with the one examined, either anatomically (*i. e.*, the adjoining cavity) or in relation to the disease.

In associating the Systems with the Internal Regions or cavities, the consideration must be specially kept in view, that only the central organs of a system are contained within a given cavity, while the other parts of the apparatus are distributed throughout the body. Thus, while the brain and spinal cord are lodged in the cranio-vertebral cavity, the various nerves take their course throughout the organism. Similarly, the heart is lodged in the thorax, but the arteries and veins ramify in the trunk and limbs. The chief digestive organs are situated in the abdomen; but the mouth and œsophagus are extra-abdominal; just as the nose, larynx, and trachea are extra-thoracic, though really belonging to the lungs, which fill the lateral portions of the chest. Hence arises the necessity (as indicated in the scheme) of dividing each System into two sets of parts. 1st, The peripheral parts, which are placed external to the region or cavity; and 2d, The central parts lodged within that cavity. The examination of the peripheral parts is

most conveniently taken first, as usually few particulars require notice; and this quite naturally leads up to the more careful and extended investigation of the central organs. Thus, in heart disease, it is the most convenient order to note the radial pulse, and the state of the veins, before investigating the præcordial region; and in hemiplegia, *e.g.*, after completing the external regions, it follows, in a kind of regular sequence, to examine the motor and sensory phenomena in the affected limbs before proceeding to investigate the condition of the brain, etc. In practice, this will be found to present no difficulty. The region suggests the physiological system; the central organs of the system suggest its peripheral extensions, while the convenience of readier access will usually indicate the propriety of taking the peripheral before the central parts. Of course, if the central organs exhibit striking phenomena, we may proceed at once to their investigation, and take the peripheral parts afterwards, but this is exceptional.

Another distinction remains to be pointed out. In investigating every System of organs we have to observe two orders of phenomena, *viz.*, symptoms (functional changes), and physical signs (material changes). These must always, if possible, be kept separate. Generally, it is most convenient to take the symptoms first, and the physical examination subsequently; and, accordingly, this is the order indicated in the Table. A careless mixture of symptoms and signs leads inevitably to confusion and inaccuracy.

With these general explanations, the directions in this part of the Table are, I trust, sufficiently intelligible and explicit.

III. *Diagnosis.*—Under this division are to be entered, not merely the name of the disease or diseases as far as made out, but the chief groups of symptoms (nosological diagnosis), and their lesions (pathological diagnosis) are to be briefly enumerated. In short, the results of the whole previous inquiry are summed up in a few brief technical expressions. The relation of succession or dependence of the various disorders or lesions may also be indicated; and the degree of certainty of each item of the diagnosis may, when necessary, be expressed. When, in the subsequent progress of the case, new facts arise, modifying or altering the diagnosis, or when new diseases supervene, a new entry as to diagnosis will require to be made.

The advantage of obtaining a brief *résumé* of the case, under the head of diagnosis, is very great. It obviates the vagueness which often arises from the number of details brought out in the Regional examination, and it concentrates the attention on the leading points of the case.

IV. Under *Prognosis*, the immediate result, and the probable remote effects—*e.g.*, tendency to relapse, etc.—of the disease should both be considered.

V. The principles according to which the *Treatment* is to be conducted, and the particular indications, should be noted here; the

direction as to diet and regimen, and a copy of the particular prescriptions ordered, must also be duly entered.

VI. *Progress of Case.*—The preceding subdivisions include all that can be done at the commencement of a case. So long as a patient continues under treatment, however, renewed observations require to be made. These should be conducted on the same plan—i.e., of regions and systems—recommended for the first examination of the case. Particular attention must be directed to the important morbid phenomena summarized under the diagnosis; but, at the same time, a careful lookout should be kept for any important changes or new phenomena which may arise. If well kept, these reports of progress form the most instructive part of the case.

VII. *Termination of Case.*—Before a case is concluded, the previous entries should, if time permits, be revised, and any omissions supplied. A careful entry should be made of the state of the patient at the time of dismissal. If the case proves fatal, and an autopsy is held, the report of this should be entered. If the report of the case is kept in a hospital journal or case-book, the reference in the index should be attended to. If the report is a private one, reflections and comments may be added at the end of the case, or may be inserted within brackets under any of the dates, when special phenomena have been observed. Throughout the whole course of the investigation, the results of the observer's experience, reflection, or reading should be brought to bear on the various incidents as they arise; and although these reflections need not always be explicitly stated, their effect will be apparent in the examination of particular phenomena, or in the modifications made in diagnosis or treatment.

Such is an outline of the mode of procedure in the detailed investigation of medical cases. The full examination is especially required in, and adapted for, chronic cases; and as considerable time is required to do it thoroughly, discretion must be used by beginners to avoid vague and irrelevant matter and useless minutiae. It is important that the medical observer should be early trained to be expeditious and punctual; to put plain questions, asking one thing at a time; to be expert in manipulation; to be brief, precise, and simple in narration; so as not only to save his own time, but especially to spare the patient the fatigue and possible injury of a prolonged examination. If all the important points of the case cannot conveniently be overtaken at once, some of them may be postponed to a subsequent occasion. In obscure or important cases, renewed examinations are always required at proper intervals. In acute or urgent cases, on the other hand, only the most accessible facts can with propriety be obtained; and in regard to the *status præsens*, a brief record of the general aspect, temperature, and state of skin, number of respirations, and rate of pulse, together with particular prominent symptoms and indications of treatment, is all

that should be attempted. For fevers or other epidemic diseases, special schemes or charts may easily be constructed. But the abbreviations of the method of examination required for urgent cases on the one hand, and, on the other hand, the more extended directions for the examination of particular regions in cases of obscure disease, need not be entered on now. When the general plan is understood, the necessary variations may easily be made. Perhaps, at some future period, I may recur to that part of the subject. For the present, omitting all discussion of the advantages and disadvantages of "systems" or "methods" in general—omitting also any discussion as to the possibility of substituting a nosological method in place of anatomical or physiological arrangements,—I have aimed only at giving an account of an anatomico-physiological method, which, simpler in use than it appears in description, has seemed to me, as a whole, well adapted to beginners, and, in whole or in part, capable of useful application in the investigation of important cases in private practice.

Part Second.

REVIEWS.

Sussex County Lunatic Asylum, Haywards Heath. Thirteenth Annual Report, for the year 1871.

Appendix B. to the Fourteenth Report of the Sussex County Lunatic Asylum, Haywards Heath. Containing Clinical Notes by the Medical Officers, S. W. D. WILLIAMS, M.D.; R. GREEN, L.R.C.P.E.; A. K. NEWTH, M.B. Together with a Paper on the Connection between Atmospheric Conditions and certain States of the Insane, by the Chaplain, Rev. T. E. CRALLAN, M.A., 1872. Lewes: 1873.

Annual Reports of the Royal Edinburgh Asylum for the Insane; of the Crichton Royal Institution and Southern Counties Asylum; of the Belfast District Asylum; of the Fife and Kinross District Asylum; of the Stirling District Asylum; of the Argyll District Asylum; of the Cumberland Lunatic Asylum;—all for the year 1872.

Thirteenth Annual Report of the State Lunatic Asylum. Utica, N.Y. Albany: 1873.

Fourteenth and Fifteenth Annual Reports of the Nova Scotia Hospital for the Insane. Halifax, N. S.

Reports on the Hospital for the Insane, Gladesville, New South Wales, for the years 1869, 1870, 1871, 1872.

It would not be very easy to write reviews upon all the reports of lunatic asylums sent to us. We have looked over a large number

of them, and though we cannot indulge in much commentary or criticism, we extract a few passages which give the experience of the Medical Superintendents of Asylums upon points of importance bearing upon the treatment of the insane. We do not mean that the reports selected for notice are the best written reports, but that they contain observations upon medical subjects likely to be interesting to our readers.

The Appendices of the Reports of the Sussex Lunatic Asylum form a series of clinical notes which are well worth reading. The medical officers record their experience in a concise and readable form, upon such subjects as the use of the Calabar bean in epilepsy and progressive paralysis of the insane; the use of bromide of potassium in epilepsy, and the influence of sedatives on the weight of the body. They have tried ergot in insanity as recommended by Dr Crichton Browne, but without any advantage. They found nitrite of amyl when inhaled useful in shortening the fit in epilepsy.

In Appendix B. the physicians give some interesting observation upon the use of electricity in insanity. They found extract of cannabis Indica in half-grain doses night and morning very useful in migraine. They give a favourable report of the benefits derived from wet-sheet packing, especially in acute mania. They complain that, by a new decree of the Commissioners in Lunacy, they are compelled to record under the heading of restraint each time that this curative agent is employed, after it had been carried on for ten years with the full knowledge and consent of the said functionaries.

There is a strong tendency, much lauded by Commissioners in Lunacy, to give more freedom to the insane than the interests of the sane will allow. One doctor writes a paper declaring seclusion to be immoral. He puts his excited patients into airing courts instead of dark rooms; another can do even without airing courts. "Seclusion and the use of airing courts," says Dr Rutherford, in the Ninth Annual Report of the Argyll District Lunatic Asylum, "may be considered to be completely abolished in this asylum. With the extent of land possessed, and an adequate staff of attendants, the most excitable, violent, noisy, or destructive cases can be better, though perhaps not so easily, treated, without having recourse to such expedients, which, if at all permitted, are so liable to be abused. It is easier for attendants to quieten a violent maniac by shutting him up in a small, dark, and perhaps padded room, than to walk with him about the grounds for several hours. The effects, however, as may be readily perceived, are much in favour of the latter mode of treatment. Airing courts serve much the same purpose out of doors that padded rooms do in-doors."

We suppose a wide Highland moor, with a swift-footed gillie for a keeper, may, at least at Lochgilphead, be substituted for an airing court, and that a peat moss is as soft as a padded room; but such arrangements are not so easy in a populous country. It could

scarcely be hoped for that views like these will be adopted by everybody. In the appendix to the Fourteenth Report of the Sussex County Lunatic Asylum, the medical officers remark :—

“ We are decidedly of opinion that seclusion, in certain forms of mental disease, is of the utmost benefit. We even go further, and affirm our strong belief that some cases of acute mania tend to become chronic if not kept in seclusion. Moreover, we believe that if a correct record could be kept, it would be found that in acute mania the duration of the disease would be lessened, and the percentage of recoveries increased, by keeping patients thus suffering in seclusion from the moment of their admission to the time when all the acute symptoms have subsided. We are most of us now agreed in viewing insanity as of purely somatic origin, and yet we shrink from treating it as we would any other bodily ailment. Supposing an organ other than the brain becomes the victim of disease, would any rational physician consider that the surest way to bring about a cure would be by increased action of the functions of that organ? And yet in acute mania, wherein the functional activity of the brain is most intense, thought following thought with most abnormal rapidity, we are advised to associate the case with the other patients; or we are told to submit him to the torture of being dragged round the airing courts by two attendants until he is exhausted, although we know that he is suffering from a disease that tends essentially to exhaustion, and that he will require all the physical power he is possessed of if he is successfully to grapple with the enemy who has met him in the gate. And all this that we may not defile the blank pages of the column in the ‘ Medical Register ’ devoted to the record of ‘ Seclusion.’ Surely this is decidedly helping our patient many steps down the hill that ends either at death or dementia.”

They add some experiments to show that the decrease of weight in patients kept for some time in seclusion is much less than has been represented by some members of the Psychological Association.

We have before us the Reports of the Stirling District Asylum for the years 1871-72. In these we have the views of a superintendent of good experience and ability upon the evil of crowding asylums with incurable cases in a state of senile fatuity or harmless dementia, who could as well be cared for in the lunatic wards of poorhouses or in private dwellings. Dr Frederick Skae details the cases in which he has been obliged to use restraint, and briefly explains the reasons which induced him to do so. These cases, putting the two years together, were only six in number. Two of them were confined because they removed surgical dressings; three were cases who attempted suicide; and one was a woman who gave way to furious homicidal tendencies. One of the suicidal cases is a man whose whole mind seems “devoted to the one motive of self-destruction,” and who has made a number of sudden and ingenious attempts to accomplish his object.

In his Report for 1871, Dr F. Skae remarks:—"Moreover, the suicide which happened last year, and which would likely have been prevented had I used restraint, has made me feel very timorous in these cases. It is so generally acknowledged too, that if a patient is really bent on committing suicide, in spite of all your watchfulness he is likely to succeed, that I cannot help thinking there are some cases in which one is justified in resorting to restraint instead of trusting to means which are evidently not so sure."

In the Sixth Annual Report to the Fife and Kinross District Boards of Lunacy, Dr J. B. Tuke gives his views upon non-restraint in a forcible way:—"I am convinced that restraint applied through the muscular force of attendants is much more irritating and dangerous than mechanical restraint. It is too great a tax on the patience of any man or woman to be struggling for hours with a refractory patient, who, by word and deed, must eventually wear out the good-nature of the best servant. In surgical cases its employment is an actual necessity. In regard to seclusion, I hold that a large majority of recent cases are best treated in bed in single rooms, and further, that chronic cases which are liable to violent outbursts of dangerous passion and obscene conduct and language are better secluded, than by being allowed to upset the tranquillity of a whole ward and outrage the sense of decency of respectable attendants and quiet patients. The necessity does not often arise, but when it does I have no hesitation in placing such a patient in a single room for a few hours."

Dr F. N. Manning, in his Report for the Hospital of the Insane at Gladesville, New South Wales, for the year 1869, takes a similar view.

"No report on an institution of this character would be satisfactory without a distinct statement of the practice pursued in regard to the restraint and seclusion of the patients; and I enter into this subject the more readily, because I differ from many of those charged with the care or supervision of the insane, and believe that in the adoption of absolute "non-restraint,"—in the reaction from the abominations and cruelties which, under the name of restraint, were the almost universal practice in asylums thirty years since,—a step too far has been taken, and a mode of treatment, most useful in a certain number of cases, and under proper care unattended with any evil results, has been lost to the alienist physician.

"During the year I have employed mechanical restraint, in the form of the camisole or long-sleeved jacket, in three cases, and I have every reason to be satisfied with the result.

"The first case was one in which I found it necessary to operate for a cancer of the lip. The patient, an irritable and sometimes violent man, was quiet at the time of the operation, and for one or two subsequent days, but afterwards became exceedingly troublesome, tore off the dressings, would brook no control or direction, and walked about with an irritable and bleeding sore which he con-

stantly rubbed. The jacket was used for three days, and the sore improved and ultimately healed without any bad result.

"In the second case the patient was subject to long-continued attacks of extreme destructiveness, during which he broke all windows near him, tore his boots and the stoutest canvas clothing to shreds, and walked about in absolute nudity. For days together this man was confined to his room, and for months he spent almost as much time in seclusion as in association with the other patients, whilst every kind of moral and medical treatment was tried without effect. Believing this to be a fit case for restraint, I had him placed in a jacket, and promised to liberate him whenever he would promise to try and restrain his destructive propensities. After two days he promised—was dressed in his ordinary clothes, and remained quiet for some time. At each succeeding attack of destructive furor he was again placed in the jacket, and was released whenever he would give a promise to refrain from tearing his clothes or breaking windows. He seemed to be much more contented, and always looked happier in association with the other patients, and enjoying the full liberty of the recreation ground, than he formerly was in his room, and he evidently recognised the fact that the jacket helped him to do what he was unable to accomplish without it—restrain his destructive propensities. On one occasion, at the bathing-house, when I was standing on the bank looking on, he dressed himself in the jacket after bathing, and selected me to fasten it, coming past one or two of the attendants for the purpose. Whilst fastening it I asked if he would promise not to break or tear, and he refused. The attacks of destructive furor gradually became less prolonged and less frequent; and although he is still an untidy and sometimes violent patient, there has been no necessity for placing him in restraint for some months.

"In the third case the jacket was used on two occasions only—once for one hour, and once for three hours. On both occasions a very destructive patient, who was completely deaf to persuasion or coaxing, promised to leave off tearing his clothes, and kept his promise once for a month, and once for a much longer period; and he has ever since been much more tidy in his dress, and in all respects more manageable.

"In these three cases, which are the only ones in which any form of restraint has been used, recourse was not had to it until all ordinary methods of treatment, medical or moral, had failed, and in all the success was unqualified. The jacket is kept under the care of the chief attendant, and is used only under my immediate orders."

In his Report for 1870, Dr Manning returns to the subject:—

"The experience of another year has served to confirm me in my before-expressed opinion. During the past year I have employed mechanical restraint in the form of the camisole or long-sleeved jacket in four cases, and in three of these with a decidedly satisfactory result. In the first case it was used to allow of the healing

of a wound of the lip after a surgical operation. It was resorted to only after the patient had, by removing the dressing and sutures, caused considerable disfigurement, and when he had shown a marked resentment of manual control.

"In the second case a woman wore the jacket during parts of six days between 15th February and 28th February, and during this time, and apparently solely owing to its use, her whole demeanour and conduct were changed. For upwards of two years she has been extremely and habitually destructive; any dress or other clothing supplied to her, no matter of what material, was soon in rags. She was abusive, foul-mouthed, and persistently idle; every medical and moral means was tried without beneficial result. The jacket (the reason for its use having been distinctly explained to her) seemed to break through a fixed habit both of body and mind. Still retaining the somewhat boisterous spirits which have always characterized her, she became almost at once civil and good-tempered, neat in attire, and extremely industrious. The destructive habits vanished, and she remains to this day one of the most manageable and useful patients in the Hospital.

"The particulars of the third case were given in my last report. During the past, as during the year 1869, the jacket has been occasionally used (as preferable, under the circumstances of the case, to seclusion) during attacks of extreme destructive furor; the patient seems not only to acquiesce in, but to be pleased with the arrangement, and when he on one occasion saw another patient in the jacket, complained of what he evidently considered a gross infringement of proprietary right."

Dr James R. De Wolff, in the Fourteenth Annual Report of the Provincial Hospital for the Insane, Halifax, Nova Scotia, thus gives his views upon non-restraint:—

"As 'total abstinence' is considered by the advocates of the temperance cause to be the only safeguard against the evils of intemperance, and moderate drinking is looked upon as a dangerous custom; so in the management of the insane the total disuse of mechanical restraint is the best security against tyrannical and revolting treatment."

"Dr Conolly's able and effectual advocacy of the principle of non-restraint, has not only led to its being adopted in every county asylum in England, with rare exceptions, but has influenced the superintendents of German asylums to follow, in part at least, so good an example. From one who has recently visited, for the third time, the British and Continental asylums, we learn that 'it is evident that in the countries of Germanic language there has been a very material diminution since 1849 in the use of the restraints in question.'"

As we have reproduced some arguments and examples on the one side, we add Dr Wolff's arguments on the other:—

"We have a sad instance recently of the utter uselessness of

restraint when applied to prevent acts of violence, in an occurrence which resulted fatally in one of the eastern asylums in November last. One of the patients manifested acts of violence on two or three occasions by striking a person with his fist, and on one occasion with a pail. In consequence of these acts, his hands were manacled in such a manner that it was thought he could not inflict injury on any one; but it was a mistake. He obtained possession of a heavy weapon similar to a long-handled scrubbing-brush, and by a swinging motion of his body (although his hands were fastened to a leathern belt) he struck one of his associates a blow on the head, which resulted in death two days afterwards.

"I have said restraint in this case was useless; it was worse than useless—it deceived the attendant, leading him to suppose no violence need be dreaded because the man was restrained; whereas no amount of restraint that any one would pronounce justifiable, could prevent serious injury if this homicidal patient were resolutely bent upon carrying out his design. If he had the use of his feet he could inflict severe injury that way, while there are other modes by which he could do serious if not fatal harm. His being irritated by restraint would render him a more dangerous patient, while under this indignity, than he would be while having the free use of his limbs. Watchfulness, and the presence of superior numbers, are more to be relied upon than any form of restraining apparatus."

When Dr Conolly commenced to advocate a more humane system of treatment of the insane, he found restraint employed to a great extent, and in a cruel and unnecessary way, and he was anxious to show that it could be entirely dispensed with; and this in many asylums was literally accomplished. Instead of the legs or arms of the patients being confined by a sleeve or a strap, they were, during the paroxysms of mania, held down by the hands of two or more attendants, who might abstain from hurting them as long as the superintendent was present, but who were very apt to yield to their impatience or irritation when he was away, so that it became necessary to prove that the ribs of lunatics were unusually brittle, in order to account for the very great frequency of fractures in asylums. The Commissioners in Lunacy did their utmost to discourage the use of mechanical restraint, and to decry those who continued to make use of it with some of their most violent patients; and some writers, in periodicals devoted to lunacy, were loud in condemnation of the superintendents of asylums on the Continent, because they refused entirely to give up all forms of mechanical restraint for the muscular power of attendants. Thus "non-restraint" became an object in itself, separated from the advantage of the patient and those around him. But although England may justly claim the merit of having done much to introduce a more humane treatment of the insane, there is no chance of our insular notion about totally dispensing with restraint being adopted either on the continent of Europe or in America, while, of late years, in Great

Britain there has been a steady and powerful reaction against the extreme views of the non-restraint men. Superintendents like Dr Lauder Lindsay of the Perth, Dr Yellowlees of the Glamorgan, and and Dr Murray Lindsay of the Derby Asylums, have gained the distinction of boldly stating views which were getting every year more general and more decided, that the utter abrogation of mechanical restraint in all cases of mania was a notion only fit for doctrinaires who do not understand the real exigencies of asylum superintendence.

In a notice in the *Allgemeine Zeitschrift für Psychiatrie* (1871, p. 604) of Dr Kellogg's Report on English Asylums, the writer remarks:—"As for no restraint, he holds the strict carrying out of it as a sentimental humbug (a true word) (*für einen sentimentalitäts-humbug*. Ein wahres wort). From private accounts and the most important journals of England, there is preparing a powerful opposition against the extravagances of no restraint."

In like manner, the superintendent of an asylum of 800 lunatics in France gives his views on the subject in the *Annales Medico-Psychologiques* (1871, pp. 375-376): "Whatever the English doctors may say, these means (*camisole, manchettes*) are better than confinement in a cell and the manual force of keepers. Almost constant imprisonment in dark and padded cells cannot but become hurtful to the patients in all points of view. As for the oversight and repression of keepers, this measure often gives rise to frequent struggles and scenes of disorder and tumult in the wards, for the other patients become excited in their turn. It may be retorted in defence of the system generally employed in France, that the no restraint of the English (surveillance and repression by the keepers, and constant isolation in cells) is only physical restraint in disguise."

It appears to me that the confinement in cells generally called seclusion, is not, in the English asylums, so long continued as the French superintendent believes. There is one evil which it is important to look upon in considering the question, and that is, the frequent use of narcotic drugs, such as chloral, opium, hyoscyamus, cannabis, conium, bromide of potassium, which, we are convinced, are employed in many cases to act as most hurtful substitutes for the mechanical restraint the superintendents have not the courage to use.

There is little room now left to dwell upon the other features in these Reports. That of the Royal Asylum at Morningside is principally noteworthy as being the last report of the labours of a man who did good work in Scotland, and who was widely known and deeply regretted wherever he was known,—the late Dr Skae.

In reading over the Report of the Crichton Institution, one cannot fail to be struck at the great variety of pleasant amusements and elegant and cultivated ways of improving the time afforded to the inmates of that beautiful establishment. Dr Gilchrist's attention to the medical and sanitary wants of his patients is attested by the unusually low rate of mortality, only two and a half per cent.,

in the Crichton Institution, and about four per cent. in the Southern Counties Asylum. Dr Gilchrist deplores the increasing difficulty of getting good female attendants upon the insane. "With all the outcry about unemployed females," Dr Gilchrist writes, "it seems singular that no one should crave to occupy a position than which no other seems so well fitted for a devoted Christian woman."

This reminds us that Dr W. A. F. Browne, now psychological adviser of the Crichton Institution, wrote a paper a few years ago in the *Journal of Mental Science*, entitled "Sisterhood in Asylums," showing what a fine field lay for female patience and charity in houses for the care of the insane. I never heard of anything flowing from it, and I imagine the outcry at present amongst a section of the female sex is for increased rights or diminished restraints, but not for increased duties.

Dr Stewart of the Belfast District Asylum: as usual with him, the Report is carefully written and well arranged. In one passage Dr Stewart remarks:—"In every case of mental disease there has been a *predisposing* as well as an immediately exciting cause. But this fact is nearly always overlooked, and only one element in the causation taken notice of. The other—and by far the more important of the two for the psychologist—is seldom, if ever, given. It is of practical importance not only to know the proximate, or immediately exciting cause, but also what was the *fons et origo mali*. A valuable guide to treatment would thus be afforded, and it could easily be obtained by the physician who signs the medical certificate. For a few leading questions put to the patient's friends would suffice for the discovery of the more remote cause of the insanity—whether consanguinity of parents, or hereditary taint, or what."

This is a very reasonable complaint, and it is to be hoped that those who send patients to the Belfast Asylum will attend to the request of the veteran physician.

Dr Tuke adds at the end of his Report the details of the post-mortem examinations which he has made during the year. They have been evidently made in a thorough and painstaking manner, though we doubt whether a document like this, intended for general perusal, is the proper place for communicating such studies, however valuable. We have heard that this appendix is withheld in copies of the Report intended for non-medical readers.

Dr Clouston gives the Statistics of the Cumberland Asylum during the first ten years of its existence, and brings out a number of interesting facts.

There is some good matter in the Report of the State Lunatic Asylum at Utica, New York, by Dr John P. Gray, and we purpose again referring to it.

Annual Report of the Peking Hospital for 1872, in connection with the London Missionary Society. By JOHN DUDGEON, M.D., C.M.

WE have read this Report with much interest. When we mention that 9618 *new* patients, of which number 1942 were women, applied at the dispensary—that a considerable number of indoor patients have been accommodated, with acute diseases, severe accidents, and diseases requiring severe surgical interference, and that all of these are brought more or less under Christian influence,—it will be seen that the Institution is one of great value and importance. The Report contains much that is interesting to professional readers,—a rather unusual feature in such publications. Our space does not admit of our quoting more than a single case. It seems valuable as a rare example of a natural cure under very unpromising circumstances :—

Artificial Anus.—The Vice-President of the Board of Punishments was the subject for many years of right *inguinal hernia*, and at the time of the Imperial marriage, when hurrying to discharge the duties of his important post, this *hernia* became strangulated. The family physician at once commenced a highly purgative treatment, with the view of expelling this collection of air, and succeeded so admirably that he burst the gut, and even then the *fæcal* contents did not convince him of his erroneous theory. He tried by all the show of learning at his command, such as apt quotations, the theory of the *yin* and *yang*, the confusion of the twelve roads which proceed to the various viscera, to explain the case, satisfy his patient, and hide his own ignorance and defeat—but in vain. From pubes to scrotum was one vast ulcer, the right testicle was exposed, and all *fæcal* matter passed over this sore. The adjoining parts were also much inflamed. A very viscid, black, adhesive plaster hid nearly all from view. And so matters had continued for twenty days, when I was called in. Warm water was freely used, the purgatives were stopped, and nourishing diet ordered. Under this plan, the patient rapidly and steadily improved. The septum shutting off the lower bowels seemed perfect, and all chance of keeping that part open was gone. To effect a cure of the extensive scrotal ulcer was the first object, hoping that when the artificial anus became smaller, a passage might be found into the lower gut, or, should it remain impervious, that an operation would destroy the *septum* and re-establish the continuity of the canal. After an interval of two months, when only a small opening remained, injections by the anus were tried, which brought away occasionally old *scybalæ* of *fæcal* matter, with large quantities of mucus. On one occasion, passage by the artificial anus ceased for six days, during which time the appetite failed, and there was frequent vomiting, with a hard, tense, and painful abdomen. The passage

again opened. It became again closed, and this time for about twelve days. The symptoms, however, were not distressing enough to warrant interference. I ordered a dose of castor-oil, which had the effect of re-establishing the artificial anus. During this time, some purulent matter escaped from the opening, inclining me to believe that, probably, the *septum* was being acted on by the impacted *fæces*. In any case, my most sanguine expectations were realized, when at the end of four months, two days after administering the castor-oil, the patient had the satisfaction of seeing the old passage restored, since which time it has continued pervious. The artificial anus has also closed. The official has had his head shaven, the evidence of the completion of his days of sickness. After visiting the tombs of his relatives, which his sickness prevented at the regular period, he will resume his official duties.

Dr Dudgeon adds: I have mentioned this case at length from its importance in itself, and from the important bearing which the knowledge of it may, indeed will, have in the highest circles.

St George's Hospital Reports. Edited by JOHN W. OGLE, M.D., F.R.C.P., and TIMOTHY HOLMES, F.R.C.S Vol. vi., 1871-2. London: J. and A. Churchill: 1873. Pp. 433.

As is usually the case in hospital reports by various authors, the value and interest of the different papers differ widely in degree, but in this volume there are several papers of very considerable value. The first consists of three excellent lectures by Mr Henry Lee: on Syphilitic Urethral Discharges, on Prostatic Discharges, and on Non-syphilitic Urethral Discharges. There are many excellent practical remarks and suggestions in the first and second; of the third, we unfortunately cannot say so much, as our presentation copy is deficient from the 48th to the 65th pages.

Dr Barclay contributes a most suggestive and practical paper, on the use of quinine during the alkaline treatment of rheumatic fever, so as to counteract the peculiar depression (of which phosphatic deposit in the urine is the sign) which is so often noticed during the progress of such treatment. It will be read with that respect and confidence which the teaching of a master in his own department deserves.

Of the same value is the next paper, by Mr Prescott Hewett, who also may speak *ex cathedra* on the subject of Surgical Injuries and Diseases of the Head. It is No. IV. of his contributions to the surgery of the head, and is on "Congenital Encephalocoele and Hydrancephalocoele." He describes the structure, contents, and position of these tumours, drawing the distinction between simple encephalocoeles which contain more or less of the cerebral mass, and

those hydrancephalocoeles, properly so called, which contain a portion of brain, "pouch-like," belonging to one of the ventricles, and filled with more or less fluid. The pouch-like prolongation may belong either to the cerebrum, the posterior horns of the lateral ventricles, or to the cerebellum, with its fourth ventricle, or to both cerebrum and cerebellum. The relation of spina bifida to encephalocoele is pointed out. Some very remarkable cases of protrusion are recorded. In one recorded by Dr Lichtenberg, "the protruded brain and its membranes formed a bluish red looking tumour of the size of a small fist, which, hanging out of the mouth, overlapped the chin, and rested with its base on the sternum; its pedicle was traced through a cleft palate close up to the root of the nose. Taken for a large polypus, it was tied, and the child died on the fourth day after the operation. At the after-death examination, the tumour proved to be a hernia of the brain and its membranes, which had passed out of the skull through a hole immediately in front of the sella turcica, and thence through a cleft palate out of the mouth." (P. 126.)

Some interesting examples are given of the difficulties which attend the diagnosis of such tumours, and the extraordinary and often fatal errors which have been made regarding them. They have been excised as if wens by Lallement; dissected away, under the impression that the tumour was a *supernumerary head!* by Buh-ring; treated for nævus by vaccination, by Paul Dubois; by seton, Guersant; by ligature, at the London Hospital; by caustics, as by Wagner.

Little can be done for such cases when much brain-substance has protruded through a small hole. Pressure and careful protection and watching seem the chief indications, and dealing with the fluid contents without interfering with the brain-substance itself.

Mr Pick records some instructive cases of cleft palate. His experience is that the previous division of the muscles of the palate is unnecessary and useless. He also records a case which he regards as one of partial rupture of an artery by external violence, resulting in an aneurism of the femoral artery. Digital compression of the vessel seemed at first to do good, but eventually gangrene set in, and amputation at the hip-joint preceded death by less than four hours.

A case of aneurism of the aorta and innominate artery, in which the right subclavian and carotid were ligatured simultaneously, and on the failure of this operation, galvanopuncture was performed, is very important, both as regards diagnosis and treatment. The catgut ligatures were sufficient to occlude the arteries without cutting them through.

Mr Brudenell Carter, in his paper on Contagious Ophthalmia, not only describes the proper treatment for the disease, but also, with his usual vigour and fire, shows how such epidemics in schools are the natural outcome and result of the total neglect of hygienic

precautions, poor diet, absence of ventilation, and encouragements to contagion found in workhouse-schools, and even in more pretentious establishments. He strongly advocates the appointment of medical inspectors.

Dr Ogle's elaborate paper "regarding certain influences exercised by the nervous system upon bone," is a valuable contribution to a difficult and hitherto little-worked subject. He has brought together a large amount of material, 1st, from experiments on animals; 2d, from the consideration of certain accidents and injuries to man; and, 3d, from the consideration of cases in which known defects or perverted action of nerves or central nerve organs are attended by abnormal conditions of bone. Under the first head the experiments of Van der Kolk, Schiff, and Ollier bulk largest. The second set are chiefly cases in which numerous fractures co-existed with paraplegia from spinal injury, and in which the fractures above the nerve lesion did well, while the ones below remained inert or nearly so. In the third set we have a large series of cases in which paralysis, ankylosis, atrophy, hypertrophy, and mental disorders have been associated with bone lesions.

The usual reports of the medical and surgical cases treated in the Hospital during 1870 and 1871 are carefully drawn up by Drs Cavafy and Laking, and Mr Howard. The surgical cases are well arranged and tabulated for reference, and at the end of each table a summary is given, rendering the results easily appreciable. As a sample of these and an indication of the work done, we quote the statistics of amputation for

1870			and		1871.		
Cases, 19		8 deaths.			Cases, 32		6 deaths.
Hip-joint, . . .	2	2	"	Thigh, primary, .	1	1	"
Thigh, . . .	4	1	"	" secondary, .	1	0	"
Leg, . . .	2	1	"	" for disease, .	7	0	"
Leg, primary, .	2	1	"	Knee, primary, .	1	0	"
Arm, " . . .	1	1	"	" disease, .	2	1	"
Forearm, " . .	1	0	"	Leg, primary, .	2	0	"
" for disease, .	4	1	"	" disease, .	3	2	"
Foot, . . .	3	1	"	Arm, primary, .	3	1	"
				" disease, .	1	0	"
				Forearm, primary, .	2	0	"
				" disease, .	3	0	"
				Foot, primary, .	1	0	"
				" disease, .	5	1	"

Of 41 cases of operation for hernia in the two years, 18 were males and 23 females; of these 17 died, 7 males and 10 females.

The 18 inguinal hernia were all males, and all oblique; 7 died.

20 femoral hernia were all females; 8 died.

3 umbilical hernia were all females; 2 died.

The apparently large mortality in the hernia cases is accounted for by the long duration of the strangulation before operation, the average being 49½ hours.

On the Operative Surgery of the Foot and Ankle-Joint. By HENRY HANCOCK, President of the Royal College of Surgeons of England, Consulting Surgeon to Charing Cross Hospital, etc. London: J. and A. Churchill: 1873. Pp. 476.

THIS work includes the course of ten lectures delivered by Mr Hancock at the Royal College of Surgeons during the years 1865-6. These have been corrected and added to so as to bring them up to date, and form a handsome volume, which contains much most interesting matter.

The first lecture is on the Anatomy of the Foot, its Bones, Ligaments, and Muscles, and is full of valuable information. The principal points brought out in this lecture are—

1. The author's views as to the importance of the muscles rather than of the shapes of the bones or strength of the ligaments in maintaining and preserving the antero-posterior and transverse arches of the foot.

Mr Hancock finds, in the peculiar attachment of the abductor pollicis, flexor brevis pedis, and abductor minimi digiti to the plantar ligament, a mode, not only of maintaining the position of the anterior pillar of the arch of the foot, but also of exaggerating its curvature, and thus strengthening it, just at the times when it is most strained.

2. In a very careful study, not only of the external marks on and shape of the os calcis, but also of the arrangement of the internal structure of the bone. The bone "has at one and the same time to support and contend against a double force applied in opposite directions: the weight of the body bearing down upon the astragaloid surfaces, on the one hand; the powerful leverage of the tendo Achillis, on the other. Bearing this in mind, this concave portion of the upper surface of the bone appears to labour under such great disadvantages, that we are forced to seek some compensating arrangement by which the bone is here preserved from injury, and, indeed, prevented from being crushed when these two opposing forces are in action. This is supplied by the disposition of the radiating lamellæ or pillars of the bone." (P. 31.) Altogether, this is a most interesting chapter, and conveys much of that kind of information which is useful by giving a meaning to otherwise dry anatomical details.

The Surgery of the Foot is begun in the second lecture, by a table of diseases for which operations have been performed on the foot, which is arranged in the queerest manner. It seems pretty complete; but diseases, injuries, inflammations, and tumours are mixed up in the most confused manner, and not even an alphabetical theory can explain it. The rest of the lecture is chiefly devoted to an account of Perforating Disease of the Foot, which has been little known in England, though very often alluded to by

French surgeons within the last ten years. Mr Hancock regards it as both hereditary and constitutional, and recommends immediate and free amputation once the disease is fairly established.

In discussing the operations on the foot, Mr Hancock gives the five excellent rules, which we quote entire, as a sort of text illustrating the principles on which he selects a given operative procedure.

(a.) That we should perform our operation as close to the diseased or damaged structure, and preserve as much of the foot as possible, with safety to our patient.

(b.) That, where practicable, we should cut through the tarsal bones with a saw in preference to disarticulating them.

(c.) That we should avoid the destruction of joints whenever we can do so.

(d.) That disease of one articulating surface does not of necessity demand the removal of the entire bone; as, for instance, when confined to the tarso-metatarsal joints, or to the joints between the cuneiform or scaphoid, it is not always necessary to remove the whole of the cuneiform bones, on the one hand, or of the entire scaphoid bone, on the other.

In such cases, however, the diseased portion should be removed by a clean cut made with a saw, and not bruised off by a gouge or chisel.

(e.) That whilst the openings in the skin cannot be relied upon as indicating the exact situation of the bone mischief, the existence of these openings, even if there be several, or the thickening and discoloration of the skin and soft parts, do not of themselves contraindicate operative procedure, since, as pointed out by Sir W. Fergusson, the soft parts, when relieved of the source of irritation, will speedily return to their natural condition. (Pp. 96, 97.)

Even in bad cases of joint disease, where pain is excessive, startings of limb frequent, and emaciation beginning, Mr Hancock would pursue his conservative surgery by stopping short of excision, and merely relieving tension by subcutaneous section of ligaments and tendons, and forcible putting of the limb in a straight position.

Many will disagree with these views of the author; but we must never forget to value the opinions of those who have, from long experience, learned patience, and trust in nature, even when they seem to discourage the efforts of those younger and more enthusiastic spirits who have greater faith in art and early operative interference.

Lecture III. is a generous, discriminating, and most carefully-prepared account of Mr Syme's operation of the ankle-joint, in which the method is *correctly* given, its results appreciated, and its originality vindicated. In Lecture IV., the modifications of Mr Syme's operation are described and appreciated at their true value, which certainly is not equal to the fuss made about some of them by their dotting inventors. The only really important modification, that of Pirogoff, is very carefully discussed in detail, and

compared with Syme's in very many ways. The result of his comparison is thus given:—

“Thus, as regards the occurrence of suppuration, the percentage of deaths, of secondary amputation, of recoveries and periods of recovery, the evidence of British surgery is in favour of Syme's operation; whilst as respects sloughing of the flap, it is decidedly in favour of Pirogoff's proceeding. Syme's operation appears to me to be unquestionably the best for disease, and Pirogoff's for accidents of civil life, since by it we preserve an increased length of limb. As to the greater facility of the performance of the latter, it does not seem to me to be worthy of much attention, neither presenting an amount of difficulty which cannot readily be overcome by an efficient surgeon.

“The statistics of military surgery are too incomplete to enable us to arrive at any definite conclusion; but we can all understand that perfect rest and quiet form essential elements in the recovery after Pirogoff's operation. The necessity of keeping the remnant of the os calcis *in situ*, the tendency to secondary hæmorrhage, and suppuration in the sheaths of the tendons, confirm this, and may, in no small degree, account for the disparity between the success attending Pirogoff's own patients, and that obtained by Professor Holway.” (P. 182.)

A very interesting and important letter from Professor Pirogoff, of date January 1866, follows. In it he shows a little soreness (not unnatural) at the manner in which some British surgeons have spoken of his operation.

A careful discussion of the merits of Lignerolle's (or subastragaloid) amputation then follows, in which the author warmly supports it in suitable cases. This is, indeed, the question. Most surgeons will agree that, if the bone is sound, and will certainly remain so, the retaining the astragalus retains the ankle-joint intact, and may result in an excellent stump; but, on the other hand, the risk of return of the disease in the astragalus must never be forgotten. Hancock's own modification of this operation is formed “by tuberosity of os calcis retained in integuments of heel, and brought up to under surface of astragalus, from which the inferior articular surfaces, together with its head, have been removed.” (P. 213.)

On the vexed question of the proper treatment in cases of dislocation of the astragalus, Mr Hancock says:—“In all cases of dislocation, therefore, whether simple or compound, partial or complete, I would make an attempt at reduction, especially when but little time has elapsed since the accident.” (P. 233.)

Data to aid in the formation of an opinion are given in the form of most carefully-arranged tables of all the cases in which the operation of complete or partial excision of the astragalus has been done by British and foreign surgeons.

In a very interesting chapter on Excision of the Ankle-joint,

Mr Hancock distinctly proves that to himself the honour belongs of being the first to introduce the operation of excision of the ankle-joint into the arena of British surgery. In suitable cases, his method is still one of the very best, if not the best, for the purpose; and his opinions on the advantages of preserving the foot, when it can be done with safety, are of the very greatest weight.

The chapter on Chopart's operation begins with an elaborate history of the invention of the method, and a vindication of Chopart's claims against Velpeau's criticisms. The most original and practical part of the chapter is that in which he deals with the question of retraction of the os calcis and pointing of the cicatrix. A dissection of a case shows that not only the tendo Achillis, but also the deep layer of muscles are concerned. Some cases of the author's show that subcutaneous separation of the cicatrix from the deeper parts is sometimes sufficient to render the cicatrix mobile, and the surface of the stumps soft, full, and painless. Another interesting point is the good results of the method devised by Mr Deleгарde of Exeter, in which he guards against possible retraction by transfixing the tendon of the tibialis anticus by the wire suture which unites the edges of the skin.

Space forbids further analysis, but we part from the author with the feeling that he has written the book to tell the profession something that he himself has worked out and knows better than his neighbours, not, as is too often the case, for the minor purpose of advertising himself.

Annuario delle Scienze Mediche Riassunto delle piu Importanti Pubblicazioni delle Anno. Per i Dottori P. SCHIVARDI e G. PINI. Anno III-II. 1872. Milano, Roma, Napoli, Palermo. Dottor Francesco Vallardi, Edit. 1873.

THIS is a year-book of the most important observations in Medical Science, made during 1872. It contains 460 pages duodecimo. There are also short reviews of the more important medical publications in most European languages. At the end there is a medical history of 1872; a list of prizes offered for treatises in different departments in science and medicine; and short notices of distinguished medical men who have died during the year,—the whole finished by a good index of the observations made, and the authors cited in the volume.

We have gone through the year-book with pleasure and interest. Those of our readers who may wish to keep up at once their knowledge of Italian and their knowledge of medicine will find this little volume of use. In a friendly notice of Stone's Half-yearly Abstract, which claims to be a digest of British and Continental Medicine, the editor remarks that the labours of Italians are in great part ignored. We fear there is much truth in this

complaint, which comes with all the more force that the contributions of our countrymen receive a good share of attention in the *Annuario*. The principal contributions to our knowledge from the beautiful land of Italy seem to be some new observations upon Pellagra (131 deaths are recorded from this disease in 1872 in the city of Milan); remarks upon Tarantism (*chorea major Germanorum*); we have the statistics of ovariectomy in Italy; and much attention seems to be paid to the therapeutic powers of electricity. There is an engraving of the new instrument of Dr Carletti to extract bodies from the oesophagus, which seems to meet a want much felt in surgery; also an engraving of the vagino-uteroscope of Verardini, which those engaged in special practice may find useful.

Part Third.

PERISCOPE.

TREATMENT OF DIABETES BY ARSENIC.—MM. A. Devergie and Foville (fils), on this subject, after alluding to the different modes of treatment suitable for different cases of diabetes, and even in different stages of the same case, and to the difficulties in regulating the diet so as to keep out articles capable of being transformed into sugar, point out how very few efforts are made to attack the *cause* of the disease, and how important it would be to discover a remedy which would in many cases diminish the sugar in the urine. This power they claim to have discovered in arsenic, and they support their view both by clinical results and physiological deductions. These we briefly abstract. Devergie, about twelve years ago, had to treat an excessively bad case of prurigo vulvæ. Nearly every remedy had been tried unsuccessfully. Arsenic was tried, and at the same time diabetes was diagnosed and verified. Both diseases rapidly improved under the use of the arsenic. Since this Devergie has in many other similar cases had the same results, both in cases accompanied by prurigo, and also in diabetic cases in which no prurigo existed. He, however, advises that in every case of prurigo in the female the physician should pay attention to the condition of the urine. Dr Foville (père) employed arsenic in diabetes first in 1857 with success, the patient only dying recently, and has since used it in many cases, some of whom notice the recurrence of the diabetes on the cessation of the arsenic. Encouraged by these cases, Dr Foville (fils), in 1868, expressed his strong confidence in the powers of arsenic in diabetes. Drs Jaccoud and Titon give equally favourable opinions. On the other hand, other authorities have not been equally successful, *e.g.*, Bouardel, Berndt, and Trousseau. These facts would prove that there are different forms of diabetes, in some

of which the use of arsenic is valuable, in others it is unsuccessful. The best form of the drug is Fowler's solution, and it should be given twice daily, in doses gradually increasing from one to six or seven drops; these should be continued for a considerable time, only interrupting it now and then for a few days, and then beginning again with half the dose at which it was left off. The physiological presumption on which the use of arsenic is based, is that the cause of diabetes being a permanent relaxation of the capillaries of the liver, due to paralysis of the sympathetic nerve, we must treat this paralysis; and for this arsenic seems very suitable, from the good effects of that drug in intermittent fever, in exophthalmic goitre, etc. Cahen, in a memoir which obtained a prize from the Institute, attributes to arsenic a special power in functional derangements of the vaso-motor system.—*La France Médicale*, Nos. 22, 23, 24.

RESECTION OF HIP-JOINT.—Dr Kappeler has made a study of thirty-four cases of hip-disease, of which number he had subjected twelve to excision of the hip-joint. Of these seven died, three recovered, with comparatively little interference with motion, one made a bad recovery, and the last was in process of cure. In comparing these results with those given by ordinary treatment, the author finds that 58 per cent. of those operated on died, as against 46 per cent. of those not operated on. But while after resection 25 per cent. recovered with good use of the limb, there was only six per cent. of the others in which the result was equally good. These statistics have the disadvantage of being based on an insufficient number of cases; but the work is one deserving of imitation, if we are ever to be in a position to estimate the value of resection of the hip.—*La France Médicale*, No. 31, 1872.

THE frequency of death as a result of tetanus, and the doubts as to the best method of treatment, render any notice of successful cases interesting. Dr Gasparo Bonci reports a case in the *Nuova Liguria Medica*. A lad of fifteen had a serious wound of his thumb. On the thirteenth day, after severe fatigue, he became so ill as to have to come to hospital. The wound was in process of healing. The muscles of head, neck, and back were violently and frequently convulsed, and the patient was unable to swallow fluid. After a consultation he was to have hot baths, amputation of the finger, and fifty centigrammes of the extract of opium. This was done, and the opium and the baths were repeated next day, still without improvement. The third day sixty centigrammes of opium were given, still without much effect. So next day subcutaneous injections of morphia were substituted, to the number of four, five, and even six daily, increasing from four, five, up to ten centigrammes of sulphate of morphine. Under these doses the patient began to improve, and on the thirteenth day he was put on chloral, a drachm and a half daily in divided doses. From this he gradually re-

covered. Quoted by Dr Henri Almez in *La France Médicale*, No. 27, 1873.

THE DANGER OF BADLY-MADE AND OLD CATHETERS.—This apparently trite and trivial matter is the subject of an important paper by M. Demarquay, from which we extract the following conclusions:—

In many cases patients using catheters break off pieces in their bladders, and such accidents are dangerous and troublesome. The fracture is due, in some cases, to bad manufacture; in others to the age of the instrument. Nearly all Parisian instruments are liable to become brittle by age. Also special conditions of the patient render catheters liable to break. These conditions may be either in bladder or urethra. If a catheter has been left long in a bladder which contains pus, it *sometimes* will be found to have the point altered and brittle, but this is not invariable; nor does it seem to depend on the *quantity* of the pus in the urine. What causes the difference? Ammoniacal urine does so; but there are special conditions of inflammation which still require study in this point of view. Well-made instruments, and especially those made of vulcanized indiarubber, alter more slowly; but even they do alter in time. The changes are of two kinds. In some the varnish comes off, and the substance of which this instrument is made comes to pieces, and some layers of it may even fall into the bladder; or the roughened instrument may become coated with calcareous deposit, become rough, and cause much suffering on its removal.

In other cases a catheter may, after some days, or even some hours only, in the bladder, be removed greatly altered; not so much in the part which had been in the bladder as in that which was in the urethra. I lately saw this occur after only twelve hours, in a patient who had stricture of the-urethra. From his case I made out—1st, That the alteration in the instrument was proportioned to the amount of inflammation in the canal, and to the alkalinity of its secretions; and, 2d, that the better the instruments were made the less change took place. Great differences exist in the manufacture of instruments. After a careful description of the methods of manufacturing good and bad instruments respectively, M. Demarquay points out that bad ones can be made more cheaply than good ones; and that, consequently, large numbers of dangerous instruments are made and exported. He suggests that instrument-makers should be inspected as well as druggists, and equally punished for selling dangerous instruments. In a word, in acid or neutral urine, even ordinary instruments are not liable to change, but in alkaline urine, and in proportion to the degree of its alkalinity, they do alter sooner or later, according to the quality. —*La France Médicale*, No. 28, 1873.

INTUSSUSCEPTION—RECOVERY.—G. G., æt. 2, seemed to be

passing through a typhoid fever of severity, with delirium, diarrhoea, dry tongue, and great tympanitis of abdomen, when he was seized with all the symptoms of an acute peritonitis, as if from perforation. With great pain he passed by stool only bloody serum, no faecal matters. Next day, in an excessive paroxysm of griping, he passed a coil of intestine about six inches in length, swollen and softened, accompanied by a smart hæmorrhage, which weakened the child. The pulse was thready; vomiting ceased, and next day he had a natural stool. He was ordered juice of meat and ice to suck; he was quite well in twelve days.

The piece of bowel is preserved; the invaginated portion is thickened, and coloured like lees of wine; the twisted and detached edge is narrowed to a point; the outer coil is swollen, flattened, and irregular, like a cauliflower. The central canal hardly admits a probe.—*Gazette des Hôpitaux*, No. 29, 1873.

FRACTURE OF SPINE—REDUCTION OF DISPLACED FRAGMENTS—CURE. By M. GUERMONPREZ, of the Saint Saviour Hospital at Lille.—A house-painter, æt. 31, while assisting to move a ladder, fell with great violence, about 12 feet, on pavement. On admission, a projection was found corresponding to the last dorsal and first lumbar vertebræ, and perhaps the next ones. It projected about $1\frac{1}{2}$ inches in its centre. Colour of skin was not altered, but the spot was painful. Movement of lower half of body was entirely lost. Anæsthesia was also complete, except in the outer and upper parts of the thigh. Reflex action could only be elicited in the muscles of the foot, not in the front of the leg. The bladder and rectum were also paralyzed; slight priapism existed. The symptoms pointed at compression of the spinal cord, the result of fracture of vertebræ with displacement of fragments.

M. Parise, who was in charge, resolved to attempt reduction at once. The patient was laid on his belly; two strong assistants pulled by the malleoli and thighs of each leg, others maintained counter-extension; the surgeon manipulated the tumour. After five minutes' pulling, M. Parise pressing the tumour with great force with the palm of his right hand, still reduction was not complete. Again extension was made, and M. Parise pressed on the projecting portion of spine with his knee with all the force he could exert. This was successful, and the displacement was rectified after fifteen minutes' exertion. Patient was put to bed cautiously, extension being kept up on shoulders and pelvis. The paraplegia was at once completely cured; sensation and motion being both restored. The bladder did not act properly till the fourth day after the accident; constipation existed for ten days. No treatment was adopted except absolute rest; after sixty-three days of absolute quiet on his back, the patient was allowed to sit up.

This case is a very rare one, and also exceedingly encouraging. When a fracture, with displacement and consequent compression of

the cord, exists, a condition which will almost certainly cause the death of the patient, it is right to attempt reduction, by which we may cure the patient without materially adding to the danger of his state.—*Gazette des Hôpitaux*, No. 44, 1873.

MEDIAN LITHOTOMY.—From a most interesting and valuable paper in the *British Medical Journal* of 11th October 1873, by Mr Cadge of Norwich, we extract the following conclusions:—Of fifty-four cases of median lithotomy, ten died. The average age of these patients was 48½ years; and the average weight of the stones removed was five drachms and five grains. Of fifty-four cases of lateral lithotomy, eight died. The average age of these patients was 46 years; and the average weight of their stones was one ounce and two grains. Thus, in similar series of cases, of very nearly the same average age, treated by the same surgeon under similar conditions, we have ten deaths by the median, and only eight by the lateral, though the size of the median stones was only five-eighths of the lateral, while, as a general rule, in lithotomy, we know the danger increases in proportion to the size of the stone. Then as to the difficulty of the operation, Mr Cadge thinks the median requires more care and skill in all cases; is *much* more difficult in children; and, in the case of an unexpectedly large stone, it is not so easy to enlarge the incision in the median as in the lateral operation. Hæmorrhage also is more frequent in median than in lateral lithotomy from the risk of wounding the bulb. In what cases, then, is median lithotomy to be advised? *Only in those patients in whom the stone is small, like a date or filbert, and in whom, for some sufficient reason, lithotripsy is not available.*

ICE AS A REMEDY IN STRICTURE.—M. Casenave recommends the use of ice in cases of retention of urine from stricture, or from enlarged prostate, and even as a mode of treating the accidents which may follow lithotomy and lithotripsy. He introduces a piece of ice of an elongated oval shape, and as large as a chestnut, within the anal sphincter, and renews it every hour. He claims for this mode of treatment the advantage of soothing pain, relieving spasm, subduing inflammation, rendering the passing of urine more easy, and thus giving the surgeon time to treat the patient by constitutional means, and prevent him from having recourse to catheterism or urethrotomy at a time when the parts are inflamed, and in a state liable to further mischief.—*Gazette des Hôpitaux*, No. 64, 1873.

CASES OF TETANUS CURED BY SECTION OF NERVES.—A young man, æt. 19, had his two lesser toes crushed by a locomotive engine. On the eighth day, the fourth toe wound was nearly healed, but on the fifth there was an indolent sore, the least touch of which caused intense pain, which extended up the leg and along the sciatic nerve. Amputation of the toe was refused. Hypodermic injections were

of no use. Tetanic spasms became most pronounced. Professor Rizzoli discovered in the wound a white filament, touching of which caused the most intense pain and spasms of tetanus. This he cut out, after which all pain ceased, the muscles regained their suppleness, and convulsions became gradually less frequent. A perfect cure followed sixteen days after the operation.

A strong adult had a gunshot wound of the left forearm. The charge was one of shot, which entered by a single wound like a ball, and, destroying the muscles, appeared under the skin close by the elbow. On the eighth day, the brachial artery had to be tied for hæmorrhage. On the tenth, an abscess had to be opened in the fold of the arm. The ligature fell on the twelfth day, and the wound was nearly healed, when tetanus came on in the wounded limb, and rapidly involved the whole body. Dr Marinelli cut out a portion of the musculo-cutaneous nerve. The spasms still continued for three hours, after which they began to diminish, and the improvement was so rapid, that in three days all the tetanic symptoms had completely disappeared.

Another Case of Tetanus Cured by Amputation.—Young man, æt. 20, had a compound comminuted fracture of left leg. Tetanus suddenly came on in the limb, but ceased after removal of an exfoliation. A month later, tetanic spasm recurred in the limb, with fever and shiverings, and absence of suppuration in the wound. For this amputation was performed, soon after which the tetanus ceased, and a cure was obtained in three weeks.—*Gazetta Medica Italiana, provincie Venete, translated by Dr Henri Almèz in La France Médicale, No. 29, 1873.*

DR ATLEE calls attention to the following very important practical point in the operation of ovariectomy. It is this: *immediately after making the incision through the walls of the abdomen, the index finger should be passed up to the region of the umbilicus, and if it can be swept freely across from side to side it must be within the abdomen.* This, of course, is an easy matter when no adhesions exist. It is always possible in parietal adhesions, when the finger is inside of the peritoneum. It is not possible, without the most unwarrantable violence, when the finger is between the layers of the abdominal parietes. The non-observance of this rule has led to the separation of large portions of the peritoneal layer of the walls of the abdomen, even when no adhesions existed, the operator having mistaken the peritoneum itself for an adherent cyst-wall. When, however, parietal adhesions do exist, the mistake may be more excusable and more readily made, particularly in such a case as the one just related, where the peritoneum is thickened and more strongly incorporated with the cyst-wall than with the wall of the abdomen. The most convenient and infallible test of being within the abdomen is *the ability to freely move the finger to and fro past the umbilicus.*—*Philadelphia Medical Times.*

TRACHETOMY IN DIPHTHERIA.—During the years 1861–1872 inclusive, there were performed in the department of Professor Wilms, in Berlin, 330 tracheotomies on diphtheritic patients under 16 years of age. Up to 2 years of age there were 6 operations, and all died. In the ages of 2–3 years, there were 56 operations, 15 recovered; 3–4 years, 69, 22 recovered; 4–5 years, 74, 18 recovered; 5–6 years, 57, 20 recovered; 6–7 years, 33, 15 recovered; 7–8 years, 21, 5 recovered; 8–14 years, 19, 8 recovered; total, 103 recoveries (31½ per cent.)—boys, 36½ per cent., girls, 24½ per cent. In the first two years, and between the ages 11–14 inclusive (of each 6), not a case recovered. The constantly unfavourable result in the later periods of childhood is accounted for by inferring that the relatively more capacious air-passages must suffer from a higher grade of disease before they afford sufficient indications for operation. In operating, chloroform is usually employed, all bleeding vessels, veins or arteries, are tied, and as a rule the inferior tracheotomy is preferred. As a rule, the canula could be removed between the fifth and eighth days. Practically, the indication for its removal was inferred if within 4–6 days the morning temperature was somewhat higher than upon the previous evening, and the respiration a little more frequent. When the indication was not regarded, the discharge soon became reddish—an evidence of irritation.

A pulse of over 152, a few hours after the operation, renders the prognosis unfavourable, and with a pulse of 170, the patient is sure to be lost; if less than 130, the prognosis is, with a few exceptions, favourable. A high temperature soon after the operation (39° C. and over) is unfavourable; a normal temperature upon the following day is very favourable. Sudden rise of temperature usually points to complications or a relapse.—*Bartels. Jahrb. f. Kinderhklde.*—*New York Medical Record*, September 1, 1873.

THE USE AND ABUSE OF NUX VOMICA AND ITS ALKALOIDS.
By JAMES THOMPSON, M.D., Leamington.—For the sake of form, I have named this paper on the use and abuse. Of the first, the *use*, I intend to say but little, as this is known well to all; the *abuse* will be what I desire to make some remarks on. My attention was drawn to the subject by some cases which I have had during the present year, and, as I can find no reference to it in any works on therapeutics which I have been able to consult, I have deemed the subject not unworthy of being considered in this section. I remember, when a student, spending some time in a house where homœopathy was followed, and the inmates seemed, at all hours of the day, to amuse themselves by swallowing globules and tinctures, the active ingredient of which was said to be derived from nux vomica; thus, during the day, they took a sufficient amount of this powerful tonic to feel invigorated and stimulated.

The matter passed out of my mind until recently, when an American gentleman came under my care; he had been sent to

England for change of air and scene. He had, from his account, suffered from some cerebral attack, due to excitement and overwork in commercial pursuits. His American physician had prescribed Easton's syrup in drachm doses three times a day. When I saw him, he was taking such doses several times a day, and had done so for a long time. He was in a highly nervous state, sleepless and excited in manner; he took a dose of syrup, and then felt better; but the stimulation was temporary, and then he was more depressed than ever, and he required another dose before he could attempt anything. At first, I did not realize the cause of this state. Medicine and regimen were of no avail, as he continued to take the syrup. After some persuasion, he consented to give it up, although he declared he thought he would die. I advised a course of mineral water, a Turkish bath twice a week, and a shower bath every morning. In a fortnight's time he was all right, and has continued so up to the present time.

Now this patient seemed on the verge of what was very like delirium tremens, and I have no doubt that the strychnine in the syrup was the cause. He drank nothing save a couple of glasses of sherry at dinner and lunch.

Among my patients I have a highly hysterical lady of middle age, much in society, who, when she is going out, takes twenty or thirty drops of tincture of nux vomica in a little water. Formerly she took sal volatile, and she says she finds the tincture much the more potent. She is aware of the power of the drug, and she only uses it on particular occasions. When much is used, she finds herself so excitable that she takes some aperient medicine for some days and a dose of chloral at bedtime. I find, on inquiring, that a medical man, whom she met at a hotel when travelling, had first suggested it to her.

A medical friend, whom I asked if he had ever noticed any deleterious effects from the use of these medicines, admitted that every day he longed for 11 A.M. to come, as, for a long time, he had taken a dose at that hour, and that he could not get through his work without it. I made an experiment on myself to ascertain the exact effects, and, although I never take any stimulant before dinner, I soon found I could not get on without it. I took the syrup in drachm doses once daily for three weeks, and in the fourth week I found myself so shaky, with crampy pains in the abdomen and legs, that I was warned to give it up. The next week I was low and depressed, but soon recovered. I satisfied myself that it was producing much the same effects as repeated nips of brandy or glasses of sherry.

From inquiries I have made, I am satisfied that this form of indulgence is on the increase. Prescriptions containing preparation of strychnine are treasured up, and sent to the chemist to be dispensed whenever the holder feels a little low. I am much impressed with the necessity of marking such prescriptions, so that they may not

be dispensed without a countersign. Recently a patient drank at a draught eight ounces of a mixture, which she had found comforting on a former occasion; it produced most serious symptoms, and might have proved fatal had not assistance been speedily summoned and active measures taken.—*British Med. Journal*, Oct. 11, 1873.

DEATH FROM CHLOROFORM.—The following letter from Dr Galt, one of the most intellectual physicians of Louisville, gives the history of another of those casualties which have to be set down to the account of chloroform:—

On the 10th of this month (May) I was requested by a gentleman to administer chloroform to his brother-in-law for the extraction of a tooth. The patient was a remarkably handsome, well-grown lad, twelve years of age, seemingly in robust health. As I am always reluctant to give the anæsthetic for dental surgery, I tried to dissuade him from taking chloroform; but, as the operation threatened to be of a very painful character, the tooth being carious and broken off below the gum, he insisted upon its use. I accordingly accompanied him to the office of Dr C. E. Dunn, a most excellent and skilful dentist. I gave the chloroform on a napkin, holding it sufficiently far from his nose to admit of the free inhalation of atmospheric air; but before coming under its influence the youth became nauseated, and threw up the contents of his stomach, composed principally of pieces of apple which he had eaten during the morning. After waiting for the nausea to subside, I resumed the administration of the chloroform, and brought him slowly and cautiously under its influence; not desiring to produce profound anæsthesia, but simply to take away the sense of pain which would necessarily accompany the operation. Dr Dunn then proceeded to extract the tooth, and succeeded in removing one of the roots. The patient struggled a little, and complained that he felt the pain of the extraction. I then gave him a little more chloroform, his brother-in-law held his hands, and Dr Dunn resumed the operation. I kept my finger on the temporal artery while supporting his head, and watched his breathing closely.

Simultaneously with the extraction of the remainder of the tooth I felt the artery flicker, and perceived a sudden pallor and change of expression in his countenance. We all noticed this change, and immediately lifted him from the chair, holding him with his head down and feet elevated at an angle of forty-five degrees; but seeing no change I laid him on the floor and pulled his tongue forward with a tenaculum, and Dr Dunn applied ammonia to his nostrils. He responded immediately with a deep, long inspiration, which he repeated several times under the effects of the ammonia and the cold douche. After taking ten or twelve of these deep, sighing inspirations, he ceased to breathe; and, though I used Dr Marshall Hall's method of artificial respiration for several minutes, I never succeeded in arousing the action of the heart. Dr Dunn very

promptly brought a bellows, with which we carefully inflated his lungs, alternately filling and emptying them. This artificial respiration was kept up for some time, but his heart had ceased to beat. I sent in for Dr Cummins, whose office is next door to that of Dr Dunn, and he came very promptly, accompanied by Drs Wible and Leachman. Dr Cummins suggested the use of the galvanic battery, and went hurriedly for it. Dr Wible suggested the Marshall Hall method, but that I had already exhausted.

I have been told since the occurrence of this unfortunate case, that the patient was suspected to be suffering from some heart trouble; but I have never been able to learn anything sufficiently definite to justify anything more than a *suspicion* that such might have been the case. He certainly did not present the appearance of one suffering with disease of the heart or of any other organ, but seemed to be enjoying robust health. I have deemed it my duty to report the case to the profession; and, having done so as plainly as I could, I have no speculations to offer in regard to the immediate cause of death. The amount of chloroform (Squibb's) used, including that inhaled prior to his vomiting, which was of course lost, was about half an ounce. He was never profoundly under its influence, and up to the time of the flickering of his pulse and his sudden pallor his breathing and pulse were natural. The former was never stertorous or laboured; the latter was steady, regular, and full. No time was lost in the application of the means of resuscitation, as everything was in reach, the bellows being in Dr Dunn's laboratory. Dr Dunn and the brother-in-law of the poor little fellow both assisted me with great promptness and presence of mind.—*American Practitioner*, June 1873.

THE ACTION OF INORGANIC SUBSTANCES WHEN INTRODUCED DIRECTLY INTO THE BLOOD. By JAMES BLAKE, M.D., F.R.C.S.—The substances, the action of which has been described in the above experiments (*i.e.*, salts of sodium, lithia, thalium, and silver), form an interesting group, possessing, as they all do, certain isomorphous relations, and yet not so closely agreeing in all their chemico-molecular properties as some other isomorphous groups. An analogous remark will apply to their physiological reactions. When introduced directly into the blood, they all have certain well-marked resemblances; but still there are differences which serve to distinguish some of them in their reactions on the different organs. Amongst the most marked reactions they produce is the manner in which they act on the lungs. Without exception, they are all lung-poisons. They all will, when injected into the veins, either by directly arresting the pulmonary circulation, or by causing changes in the lung-tissue, rapidly prevent the aeration of the blood. They also all agree in another important respect. Some of them, notwithstanding the marked changes they cause in the blood, do not all diminish the irritability of the heart-muscles; so that these

continue to contract longer without the stimulus of arterialized. There is another property which they all seem to possess, with the exception of cæsium; and that is, that they exert no direct effect on the functions of the nervous system. In striking contrast to their action in arresting the passage of the blood through the pulmonary capillaries is the facility with which they all, with the exception of silver, pass through the systemic capillaries, unless introduced into the arteries in large quantities. Nothing can more strikingly show the want of action of these substances on the nervous system, and on the systemic circulation, than the fact that they can be introduced in much larger quantities into the arteries than into the veins without proving fatal. With the salts of thalium, for example, three and a half grains, when injected into a vein, will instantly kill by arresting the pulmonary circulation; whilst six times the quantity can be injected into the arteries without producing any marked symptoms. They pass over the heart through the nervous tissue, and find their way through the systemic capillaries without apparently causing any marked change in these organs; and it is only when injected into the arteries, in quantities large enough for them to reach the lungs in a sufficiently concentrated state, there to give rise to these well-marked effects, that they kill. These properties are found in all the more strictly isomorphous members of the group. The only exceptions met with are in the salts of cæsium and silver; the latter evidently an outlying member of the group. The former of these salts exerts a decided action on the nervous system when introduced into the arteries, and a small quantity of a salt of silver, in the arterial blood, causes obstruction to its passage through the systemic capillaries.—*Journal of Anatomy and Physiology*, June 1873.

Part Fourth.

MEDICAL NEWS.

THE OWEN'S COLLEGE SCHOOL OF MEDICINE.—The introductory lecture of the session was delivered on Wednesday, 8th Oct., in the Chemical Theatre, by Professor Arthur Gamgee. Mr T. Ashton presided; and there was a large attendance of professors of the College and the general public. The gallery was appropriated to the students, who conducted themselves in the usual boisterous manner previous to, and for some time after, the entrance of the Lecturer and the Chairman, and it was not until the latter had expressed his intention of clearing the gallery that order was restored.

The Chairman, in introducing Professor Gamgee, said he had no doubt that the reputation which the Professor had gained in

Edinburgh would be greatly increased by the work which he was about to do in that College. He came to devote the whole of his time and talent to the interests of that school; and he (the Chairman) hoped to see the time when it would be as celebrated for its results in investigation as in teaching. There was no doubt that to that end they must have not only one professor but many, who would be able to devote their time and energies to the interests of the profession.

Professor Gamgee said, in looking forward with hopefulness to the future of the Medical School in its connexion with Owen's College, he would be the last to refrain from offering a tribute of praise to the founders and promoters of what might now be called the old medical school of Manchester. It was to the honour of the teachers of medicine in this city that they succeeded in establishing in Manchester the first of the numerous provincial schools of medicine; that in the face of obstacles of all descriptions, often at great personal sacrifices of time and money, they laboured with very considerable success to further the spread of medical education. Now that the older was being merged into the newer and larger institution, it would be ungraceful and unjust on his part were he not to refer to a few of the men whose names were most honourably associated with the past teaching of medicine in Manchester. First to be named was the late Mr Jordan—(cheers)—who very early in the century became a teacher of Anatomy and Physiology in this city, and whose lectures were first delivered in the provinces, and were recognised by the medical corporations as qualifying for examination the students who attended them. In 1825 Mr Turner organised the Pine Street school, and gathered around him teachers, amongst whom were the illustrious Dalton, Sir J. Bardsley, and many others of distinction. In 1850 the Cheetham Street school, among whose teachers were the late Mr Dumville, Dr Watts, and Mr George Southam, was established; and in 1858 the two schools were amalgamated. To the energy and devotion of one of the teachers in particular (Mr Southam) the success of the Manchester School had for many years past been greatly indebted, and but for him the union of the Medical School with Owen's College might not have been effected.—(Applause.) Professor Gamgee proceeded with a review of the progress of medicine from the time of Hippocrates, in order to show that the study of medicine had at all times been influenced by the study of science. Glancing, towards the conclusion of his paper, at the present and past history of the famous medical schools of modern Europe, he said the system of centralization which had been pursued in France since the days of the First Empire had produced as marked an effect in stifling the scientific ardour as in enervating the political energies of the nation. Enforced dependence had been fatal to progress, and the attempt to centralize the intellectual power of France in Paris, for the glory of the capital, had proved as fatal as would be an attempt to concentrate

in one focus the commercial enterprise of this or any other country. The Germans had perceived the mistake of centralization, and in many departments of science had clearly asserted for themselves the right to hold the first place. The first cause of their progress was to be found in the very perfect system of education pursued in the Gymnasias of Germany, which, as a consequence, led to the average medical student possessing a greater amount of both literary and scientific knowledge than was the case with us. The second and most potent cause lay in the fact that in the numerous universities of Germany learning met with opportunities of development which did not exist elsewhere. The most ardent enthusiasts of the German unity must admit that the intellectual success of this country was greatly owing to the jealous fidelity with which the elements of German greatness had been developed in the separate centres of the fatherland, and pre-eminently in its universities. In them the greatest encouragement had always been given to the most intellectual men to devote themselves to the acquisition of learning; not merely to become acquainted with what was already known, but to lead in the path of scientific discovery. It was the scientific knowledge acquired in the physiological laboratories of Germany which had chiefly led to the pre-eminence which we were obliged to acknowledge. German physicians had, in these laboratories, learned the methods of inquiry which, when applied to medicine proper, tended every day more and more to raise it from the condition of an art to that of a science. If facts had compelled the cheerful admission, that we had much to learn from abroad, it was none the less true that contemporary British physicians had good reason to reflect with pride on the success with which they had sustained the heritage of fame transmitted by Linacre, Harvey, and Sydenham. With the instinctive sense of the useful, and the industrial acquisitiveness which characterized our countrymen, the best fruits of foreign investigators had been gathered up and assimilated by British physicians, who, science and practice combined, were, he was inclined to think, still not second to the physicians of any other country. What we did want, and what we were happily growing day by day more alive to, was a wider development of schools of science wherein competent men should have the opportunities essential for research, and young men might be trained in those tastes and habits of mind which were of the very essence of successful intellectual pursuit. Now that the wealth of such great industrial centres as this city was being utilized for the highest educational purposes, it was not too much to hope that on the scene of the labours of Dalton and Joule that school might grow to exemplify once more that happy combination and adaptation of resources which had so often placed Englishmen in the van of literary and scientific progress as of manufacturing and commercial enterprise.

THE MEDICAL SCHOOLS.—The annual registration of gentlemen pursuing their professional studies at the eleven recognised metropolitan hospitals, was brought to a close at the Royal College of Surgeons on Wednesday, the 15th Oct. It is understood that there will be a small addition to the undermentioned, by permission of the President, of gentlemen unable to reach their respective schools from illness and other unavoidable circumstances :—

	Total number registered.		Fresh entries.	
	1873.	1872.	1873.	1872.
Guy's Hospital, ...	304	319	89	83
St Bartholomew's Hospital, .	273	254	100	79
University College "	270	273	63	83
St Thomas's "	184	156	53	51
King's College "	139	120	45	35
London "	114	91	36	31
St George's "	114	108	35	47
Middlesex "	80	47	38	22
St Mary's "	65	61	22	21
Charing Cross "	41	48	13	20
Westminster "	24	19	10	4
Totals	1608	1496	504	476

From the above it will be seen that there is an increase of 112 in the total number, and of 28 in the new entries over the number registered last year.

The following table, showing the fluctuations in the registration during the last ten years, will no doubt be read with some interest by metropolitan teachers and others taking an interest in medical education :—

In 1864 the number of students registered was . . .	995
1865 " " " . . .	1013
1866 " " " . . .	1027
1867 " " " . . .	1125
1868 " " " . . .	1194
1869 " " " . . .	1241
1870 " " " . . .	1298
1871 " " " . . .	1475
1872 " " " . . .	1496
1873 " " " . . .	1608

The returns from the provincial schools have not yet been received. In both returns a margin must be left for several gentlemen still continuing their studies for the Fellowship of the College of Surgeons, which necessitates a more extended curriculum. There is, we are informed, an increase in the number of students pursuing their studies for the diploma in dental surgery.—*Medical Times and Gazette.*

IN a recent number of the *Gazette des Hôpitaux*, attention is directed to the history of a young French doctor, who met with his death under peculiarly touching circumstances. In August 1870, Dr Lagrelette enrolled himself as a medical volunteer in the Société de Genève, and immediately after distinguished himself before Metz by his devotion to the wounded, abandoned there by Marshal Bazaine. His conduct on that occasion met with the highest praise from his superior officers. In the October of the same year he joined the regiment of volunteers commanded by Colonel Keller, as surgeon-major, and a month later Captain Lauch becoming ill, he took the command of his company. On the 16th January Colonel Keller's regiment was attacked before Hericourt by a greatly superior force. After a sharp struggle, when those who had escaped the Prussian fire gathered themselves together, Dr Lagrelette was informed that his sergeant-major had been left wounded on the field of battle. Unheeding the earnest entreaties of those around him, he at once returned to the scene of combat, succeeded in finding the wounded man, raised him in his arms, and was carrying him away when a ball struck him on the temple. Dr Lagrelette fell at once, and expired a few minutes afterwards.—*Gazette des Hôpitaux*, No. 78, 1873.

SKIN-GRAFTING.—A young lady who had her scalp torn off by an accident in New Haven recently is reported to be doing finely. The *Journal and Courier* says, "The physicians have found thirteen persons willing to part with a small piece of their cuticle, and the medical gentlemen have placed the contributions on the head of the patient with gratifying results. As one after another volunteers to let the doctors clip off a piece of skin, the number of those willing to do likewise increases, and it looks as if the girl would have a scalp and a head of hair again. The younger physician attending first set the laudable example."—*Philadelphia Medical Times*.

PERINEORRHAPHY. (*To the Editor of the Medical Record*).—Sir,—In perineorrhaphy I use, instead of metallic wire, a suture of horse-hair. Take two horse-hairs, each sixteen inches long; pass them through the eye of a large semicircular needle; bring the four ends together and tie in a single knot. Arm in this way as many needles as you intend to introduce sutures. Then proceed as in the ordinary operation, using Dr Sims's matchless needle-holder: cut off each strand, after its introduction, close to the eye of the needle, and knot the end. Insert the sutures pretty close together, say half an inch apart. After all are in place, bring each pair of ends together and tie in a surgeon's knot, making the common interrupted suture. For superficial stitches a single horse-hair answers admirably. Horse-hair is easier of management than silver wire; it causes no suppuration along its course; it does not chafe or

excoriate the external genitals; it is not liable to catch in the patient's linen; and when its office is ended it can be withdrawn painlessly. I have never used it in the quilled-suture.—E. S. BUNKER.—*New York Medical Record*, 15th Aug. 1873.

BRUNETTI PROCESS.—The "Brunetti process" for the preservation of the dead consists of several processes. 1. The circulatory system is cleansed by washing with cold water till it issues quite clear from the body. This may occupy from two to five hours. 2. Alcohol is injected, so as to abstract as much water as possible. This occupies about a quarter of an hour. 3. Ether is then injected, to abstract the fatty matter. This occupies from two to ten hours. 4. A strong solution of tannin is then injected. This occupies for imbibition two to ten hours. 5. The body is then dried in a current of warm air passed over heated chloride of calcium. This may occupy two to five hours. The body is thus perfectly preserved, and resists decay. The Italians exhibit specimens which are as hard as stone, retain the shape perfectly, and are equal to the best wax models.—*Ed. J. Hallock, in Journal of Applied Chemistry*.

THE Middletown (N.Y.) Physicians have organized themselves into an association for the purpose of protection against "imposition by a class of people who never pay their physicians' bills, and, without any other reason than to evade such payment, change from one physician to another and thereby escape." The secretary of the association is to keep each member informed of all persons who habitually refuse or neglect to pay their bills, and these will have to get along "without benefit of" doctor, or else pay "cash strictly in advance" for attendance.—*New York Medical Record*.

VARIOUS THEORIES.—After the meeting of the American Association for the Advancement of Science, "Section 2" went in for a little fun. One member, remarking on the frequency with which naturalists were mistaken in their first impressions, approached the blackboard and drew a fragment of bone, which, he said, a friend, a naturalist, had reconstructed an animal from. The reconstruction was effected on the blackboard, and something like a terrific tom-cat was the result. Meanwhile, he described the bones and parts as he added them, drawing and speaking at the same moment. Then he suggested certain anatomical objections and possible improvements, which produced an amusing change in the figure. Finally, he said, they concluded to restore the fragments on a different hypothesis. The fragment was drawn again, and in an instant a few strokes drawn additionally revealed its true character. It was the handle of a jug. There is a wise lesson in that joke.—*Philadelphia Medical Reporter*.

CORRESPONDENCE.

To the Editor of the Edinburgh Medical Journal.

DEAR SIR,—During a residence of more than thirty years in East India, I had frequent opportunities of treating Asiatic cholera.

It has lately appeared in several parts of continental Europe as an epidemic—one or two suspicious cases have been reported in England. Should it become epidemic in Great Britain, I offer for publication a circular which for many years during the latter part of my Indian service I was in the habit of distributing to my patients, also to the many medical officers in my circle of superintendence.

It contains clear and simple instructions for the early treatment of that disease; I believe that it is only during the "first stage," the pulse being still distinct, that it can be cut short, and prevented progressing towards the "stage of collapse." *Practically* I have found the treatment recommended of great benefit, the disease having frequently been arrested before the medical attendant had time to appear. *Morally* the possession of some such simple directions may prevent the access of a malady, which, as is well known, is most likely to seize on those who fear its attack.

In East India, congestion of the kidneys, and consequent suppression of urine, is an invariable symptom of true cholera morbus; in fact, I consider this symptom diagnostic of Asiatic cholera as distinguished from English cholera, or what is styled choleraic diarrhoea.

Per se, suppression of the renal secretion is frequently so fatal, producing uræmic poisoning, coma, and death, that I believe the powerful counter-irritation I recommend, and have for many years practised, is one of the chief causes why my treatment of the early stage has so often arrested what certainly appeared to be the commencement of a case of "true cholera."

Of course, it can be said from the early symptoms being removed that possibly the case was not one of cholera morbus; but as the success has followed the treatment even in the early days of epidemics, when usually (at least in East India) almost every case ends fatally, I hold the conviction that my remedies were successful in cutting short the disease; and every medical man will admit that to prevent or remove suppression of urine, must greatly tend to increase the chances of cure, also that the counter-irritation prescribed can in no way aggravate the disease.

In treating cholera, sinapisms are constantly applied to the chest, abdomen, and legs; I have never found them of the slightest benefit in such localities.

Curiously, they have never, as far as I know, been placed directly over the kidneys, although, in the report on cholera of the London College of Physicians (I write from memory), several hundred autopsies are noted, and in each was found congestion of the kidney

The stimulant (any stimulant will do) must be one in reference to the capacity of the patient.

In our books certain amounts of stimulants are mentioned as the average dose; but in cholera, to arrest the first stage, it must be proportioned to the drinking habits of the patient—a wineglassful of brandy will be a strong stimulant to one unaccustomed to the use of spirits, but of not the slightest use to a man who habitually and daily takes several glassfuls, perhaps in addition to wine and beer.

The doses I recommend are therefore stated in a graduated form.

The addition of opium I think useful; but, as noticed in the circular, neither spirits nor opium must ever be administered if the patient, when first seen, is in the second stage.

I cannot assert that I know of any cure for the stage of collapse, but something must be done, and what I have recommended has been my usual practice. The counter-irritation over the kidneys may even then be beneficial, and the *hot* saline enemas, if regularly and continuously persevered in, sometimes appear to restore heat and circulation.

The idea of these is not mine; I must have read about them; but it is so many years ago that I am unable to give credit to the author of a remedy which I consider more useful than any other in extreme collapse.

I may remark that it has been most successful in private cases; in a crowded hospital it is impossible to administer them with sufficient frequency or regularity.

You will observe that I have restricted myself to the early symptoms and treatment of cholera morbus when epidemic. Of its causes, pathology, etc., I know nothing, even after so many years. I would be glad if, by the extensive circulation of my "directions" for early treatment, when cholera appears amongst us, some of my countrymen were enabled, if medical assistance was not speedily procurable, to use remedies which I am convinced do check the disease, if taken exactly as directed, on appearance of the symptoms described.

In India I found that the simplest method to distribute the circular was to request the editor of a newspaper to issue, once, a copy with each number, and being printed on coloured paper it was more readily noticed.—Yours faithfully,

GEO. R. PLAYFAIR, M.D.,

Late Deputy-Inspector-General of Hospitals, Bengal Medical Service.

CIRCULAR REFERRED TO BY DR PLAYFAIR.

WHEN CHOLERA IS EPIDEMIC.

Avoid eating any green vegetables or much fruit. Never use strong purgatives, especially those which are called saline, such as Epsom salts.

Cholera is curable in its early stage, but this stage is frequently not noticed, owing to ignorance of the symptoms. These are:—1. Disinclination for food. 2. Sense of depression and fatigue. 3. Feeling of relaxation, sometimes pains

and cramps in the bowels. 4. One loose motion (perhaps only one), followed by others, each more loose than the preceding. 5. Nausea at stomach. 6. Vomiting first of contents of stomach, afterwards of a colourless watery fluid. The pulse quite distinct. Any two or three of these symptoms, when cholera is committing its ravages in a district or city, should be looked on with suspicion, and the remedy taken. Symptoms 2, 3, and 5, are difficult to perceive in children; in them, a contracted appearance of the features, and darkness under the eyes, in addition to the purging and vomiting, ought to give suspicion of cholera.

REMEDIES.

EXTERNAL.—As early as possible, a very large mustard poultice, having mixed with it a heaped teaspoonful of cayenne pepper, to be placed across the small of the back so as to be over each kidney, and to be kept on at least one hour for an adult male.*

INTERNAL.—1. *For an adult accustomed to take several glasses of wine or beer daily*:—Rum or brandy, two wineglassfuls. Cayenne pepper, four grains (two pinches). Laudanum, forty drops.

2. *For an adult unaccustomed to any stimulants (male or female)*:—Rum or brandy, one wineglassful. Cayenne pepper, two grains (one pinch). Laudanum, twenty drops.

3. *At fourteen years of age*:—Rum or brandy, half a wineglassful. Cayenne pepper, one grain. Laudanum, twelve drops.

4. *At six to eight years*:—Rum or brandy, one dessertspoonful. Cayenne pepper, half a grain. Laudanum, six drops.

5. *At two to five years*:—Brandy, one teaspoonful. Peppermint oil, one or two drops. Laudanum, three drops.

The above doses to be mixed with a wineglassful to a tumblerful of as hot water as can be swallowed, and the whole to be taken at once.† It is essential that the patient keep quiet in bed and in a darkened room. In hospitals, and other places where many are living together, it will be convenient to have a bottle or bottles ready of cholera mixture, to be made thus:—

Brandy or strong rum, ...	1 bottle.	} Mix.
Laudanum, ...	320 drops.	
Cayenne pepper powder, ...	32 grains.	

Dose for an adult, 2 wineglassfuls (if accustomed to stimulants).

Or ditto, 1 ditto.	} To be given mixed with hot water, as above.†
14 years, $\frac{1}{2}$ ditto.	
6 to 8 ditto, 1 dessertspoonful.	
2 to 5 ditto, 1 teaspoonful.	

Also under such circumstances instead of mustard and cayenne pepper, use two parts of coarsely-ground flour or oatmeal, and one part of cayenne pepper made into a poultice, to be used as above.* It is further advisable that persons in the early stage of cholera should not be treated in the same room with those in whom the disease has advanced to the stage of collapse—in which the pulse is almost imperceptible. *If the first dose is rejected, give a second.*

When there is no pulse perceptible, or it is very weak, the above mixture must never be given. **THE TREATMENT SHOULD BE:—**

1. Mustard and cayenne pepper poultice. 2. Saturated solution of camphor in spirits of wine. Dose.—One to three drops in water, every quarter of an hour. 3. Hot water enemas (large tumblerful as hot as can be borne), having dissolved in them half an ounce of common salt, and thirty grains of carbonate of soda, to be injected every half-hour and retained as long as possible. These are useful in restoring warmth, and the pulse, in cases of extreme collapse. 4. As soon as the pulse begins to be felt and the urine to be secreted, give at short intervals small quantities of light easily-digested food.

N.B.—In every case send at once for a Doctor.

OBITUARY.

DEATH OF DR JOHN MURRAY.—To his friends in Edinburgh—and they were all who knew him—the death of Dr John Murray comes with a suddenness and unexpectedness that leaves us almost too stunned to realize it at all. He was so bright, and young, and full of life, his mind and body in such happy equipoise, doing his work as if it was a pleasure, and finding his pleasure in change of labour. He was so evidently marked out for a successful, fully-rounded, active life, and he seemed in a very rare and uncommon way to have found his place in the world, and to like it, that we can hardly realize it is all over. We transfer to our pages the eloquent, feeling, and fitting tribute which the journal on which he worked so well has paid him, and we would sympathize with its staff on the loss which they have sustained. The Middlesex Hospital has lost a most promising teacher, and still worse, a manly, warm-hearted, friendly Dean; the profession, a physician who would have been one of its brightest ornaments. To us, his old friends and teachers in Edinburgh, the loss is to each of us a personal grief. We shall miss his constant courteous help, his cheerful friendliness that seemed never to weary. We have lost a bit of Scotland out of London. Few men ever made so many friends, still fewer could keep them so well. Wherever there are young Edinburgh and Aberdeen graduates (and where are they not?) there will be genuine grief when the news of John Murray's death is told.

"IN MEMORIAM.—JOHN MURRAY, M.D., M.R.C.P.—Dr John Murray, Assistant-Physician to the Middlesex and the Children's Hospitals, died on Wednesday, aged 29, after an illness of three days, from an attack of hospital sore-throat, complicated with sudden and severe oedema of the larynx, which necessitated tracheotomy. The announcement of the sudden and premature death of this gifted, energetic, and much-beloved young physician, will be felt as a source of personal grief by a very large number of our readers; and there are few who do not know how great a public and professional loss it bespeaks. We cannot well speak of the story of his life and the circumstances of his death in words which seem fitting to convey a full sense of the loss sustained; for it involves to us, as to others bound to him by ties of intimate affection and close association, a grief sacred from words. But his life has been pregnant with lessons which we all need to learn, and his example was one which may well be recorded as an eminent encouragement to young men of worth and high endeavour; and his story must not be left wholly untold in these pages.

"John Murray received his early education at Aberdeen, where his father, Mr Andrew Murray, holds a highly-respected position as

advocate. His career at school and college was marked chiefly by traits of generosity, courage, and tenderness, which bespoke the future development of his character. Manly, well-grown, active, and intelligent, he was always a favourite, if not always the most distinguished, student of his class. At school he was not specially distinguished in his classes, except in botany and natural history, which required outdoor work. After passing through a complete course in Arts, and graduating M.B. in Medicine, Murray came to London, and began a further course of preparation for medical labours at Paris and Berlin. He was distinguished at Middlesex as one of the most able, zealous, and reliable resident officers who had ever entered the hospital; and, when he quitted the indoor service, he had entirely won the confidence of the physicians. His introduction to the staff of this Journal was due to the recommendation of Dr Murchison and Dr Stewart, whose good words he quickly and amply justified. Commencing his connexion with the Journal in 1867 as one of its hospital reporters, he gradually advanced into more intimate and important association with its work; and for the last few years, as one of its sub-editors, and as a large contributor to its columns, he has been closely identified with its utterances and efforts on most of the questions of the day. He was imbued with a natural ardour for justice and truth, and could never rest content if he witnessed or heard of any wrong done, of any good work left undone, or of any good man struggling with adversity, without throwing himself into the struggle, and battling for what he believed to be right. He had the rare gift of battling with vigour but without bitterness. If he inflicted a wound, he left no sting behind; and, though his nature was vigorous and combative, his manner independent, his opinion outspoken, yet his natural kindness of heart appeared in all his bearing; he gave little offence; and even his opponents recognised the generous nobility of his motives, and were well disposed to find an opportunity to become his friends. In a career so short and so prematurely arrested, there have been few opportunities for high achievement; but those opportunities were nobly used. Dr Murray had rendered great services to the Volunteer medical officers; he has vindicated the right of the graduates of Edinburgh to criticise appointments to the professorial chairs; he had rendered valuable aid to an institution of great usefulness, the East London Hospital for Children, once struggling for existence, now well established; he had accepted and courageously fulfilled important missions for this Journal; and the reports on the Royal Free Hospital, the Westminster Hospital, on the typhoid fever at Scarborough (on the occasion of the Prince of Wales's illness), and other important articles, testify to the courage, ability, and honesty with which he habitually fulfilled whatever duties he undertook. His native town, Aberdeen, is indebted in no small measure to the strong protests which he indited in these columns for the recent provision for fitting

accommodation for its fever patients. During the Franco-German war, he spent the autumnal recess from his hospital labours in the ambulances around Sedan, and he won there the friendship and lasting esteem of many officers of the Red Cross Society, by his earnestness of purpose and unselfish devotion. Some little of his public work is known, and may thus be recorded; but hundreds are indebted to him for a kind word and a helping hand, for defence from wrong and for encouragement in the right, for friendship, for words of cheer, for sound reliable advice, and for delicate and substantial acts of kindness.

"His career at the hospitals was one of early and rapid advancement, of excellent achievement, and yet higher promise. As Medical Registrar he made a mark at the Middlesex Hospital; and, attaining early promotion as Assistant-Physician and Lecturer on Pathology, he became one of the most active, influential, and useful officers of the hospital. Much beloved by the students and by his colleagues, he was lately appointed Dean, and had the gratification to see that his exertions were already palpably adding to the increasing efficiency and reputation of the hospital and school. When a vacancy occurred at the Children's Hospital, Great Ormond Street, he was by common consent selected as Assistant-Physician out of many eligible candidates.

"Well known and much esteemed by a wide circle of professional friends, thus at the early age of 29 he stood in a position of enviable usefulness, prosperity, and promise. Physician to two important hospitals, Dean of a distinguished medical school, wielding no small influence in the medical press, beloved by a most numerous circle of friends, whom his merit had won, and whom his amiable and steadfast character attached to him, he had already, as it were, passed the threshold of a career which included in its present privileges and near prospects all which his heart most desired, and all which good men hold most dear and honourable.

"His love of out-door exercise was great, and if at his busiest seasons a friend proposed a long walk or a row on the river, he was generally ready to go, and was the most indefatigable of the party. His energy was overflowing, and with all his work in London, he sought these other opportunities of expending it. He had concluded a season of hard work by a holiday in Norway during the month of July, from which he returned bronzed and invigorated, and only too ready to assume an ample load of labour. During the vacation months he remained in London, doing not only all his own work, but that of many others who were absent. On Thursday last he expressed himself as well, but fagged; on Friday he saw his patients and was partially at work, but complained of hospital sore-throat; on Saturday he was suffering so much from inflammation of the fauces and soft palate that he stayed within doors, and had the advice and treatment of medical friends, which considerably alleviated his symptoms. He was seen repeatedly on

Sunday, when suitable inhalations and other means had much relieved the pain and swelling about the fauces, which were the only prominent symptoms. Early on Monday morning he was seen by Dr King and Dr Morell Mackenzie; and, while Dr King was still with him, he was suddenly seized with spasm of the glottis (which was now very cedematous), producing asphyxia so alarming as to be nearly fatal at the time, and to necessitate instant tracheotomy, followed by protracted artificial respiration to restore animation. He rallied fairly after the operation, enough to excite hopes of his recovery; but they were fallacious. During his short illness, he was surrounded by medical friends, who were devoted in their attentions. Dr Morell Mackenzie, Dr King, and Mr Morris were in immediate attendance, but he was seen also by his friends Dr Murchison and Dr A. P. Stewart; and there were never wanting those of his friends and colleagues who felt it a sad but precious privilege to watch by his bedside during all the hours of night and day, and strive to snatch from impending fate a life which was dear to all. It is seldom that any event can have caused so general and deep sympathy, or that the death of any physician has been felt by so many as a personal loss, and a ground of public regret. This is the more remarkable, as Dr Murray was still a very young man. But exceptional opportunities, created by his own meritorious effort, had early afforded him opportunities of being personally well known to every practitioner of eminence in the metropolis, and to many in the provinces; and none could know him without seeing how true, how generous, how gentle, how unselfish, and how earnest he was for public ends; how willingly he spent himself, how much he loved his profession, and how much he loved his brother men; so that there are none who knew him who will think this eulogy strained, although it may well fail to define the subtle charm and deeper qualities of him whom it weakly commemorates. A relative of Dr Murray writes: 'The blank caused to his numerous relatives in London is irreparable. He had his national love for the "Clan" developed to a high degree, and we shall miss for years to come that vigorous kindly face, which has assisted so often at our rejoicings and mournings.' His example will live as one which testifies what simple worth and manly courage may ensure of solid enduring achievement in early manhood. His sudden removal from labouring in a sphere of usefulness, in which he never spared himself, is a mysterious and solemn dispensation. His memory will long be dear to many to whom his familiar presence was a daily refreshment; and probably a marble will be erected by his friends, to perpetuate the recollection of his worth, in the hospital where he worked till death overtook him. It is by the living influence of his life and memory that he would most desire to be remembered, and this influence will not soon fade." — *British Medical Journal*, Oct. 1873.

NÉLATON, 1807-73.—On Tuesday, the 23d September, the body of M. Nélaton was followed to its last earthly resting-place by countless crowds of mourning citizens, and buried with the military honours befitting his rank as "Grand officier de la Legion d'Honneur. A pupil of the well-known Dupuytren, Auguste Nélaton, in his early surgical career, delighted the heart of his master by betraying signs of the inventive genius that was to render him famous in after life. We will leave to his biographer the task of tracing the career of this great surgeon, and enumerating all he has done for surgery. Though meeting with due recognition of his talents from the members of his profession, as testified by his election in 1851 to the chair of Clinical Surgery, it was not till 1862 that his successful treatment of General Garibaldi's wounded foot, and subsequently the prompt and daring operation by which he saved the life of the Prince Imperial, gained Nélaton his European reputation. Unspoiled by a career of unrivalled success, he retained to the last the courteous urbanity of manner and the kindness of heart that endeared him to high and low. He died, after some months of suffering, from organic disease of the heart, leaving a large fortune—one of the largest perhaps ever made by a surgeon.

WHAT Arbuthnot was to Queen Anne, what Sir James Clark was to Queen Victoria, Nélaton (whose death is just announced) was to Napoleon III. To the late Emperor he was much more than a body-surgeon; he was, like his two Scottish counterparts, a kind of "Aulic counsellor." For this confidential post he had many qualifications, which, indeed, were identical with those which made him the first of European surgeons. Nélaton had more in him of Wellington than Napoleon; he was less like your Simpson than your Syme. Cautious in making up his mind, though quite capable of daring when daring was required, he rarely risked failure, while his manner was a compound of suavity and firmness—the "hand of iron in the glove of velvet"—which was itself a therapeutic agent. He excelled in diagnosis—searching, taking in all the conditions, weighing them carefully, and concluding with marvellous correctness. As an operator, he was dexterous with every known instrument, as well as with those of his own devising. Cool and watchful of every symptom, he was skilled in after-treatment, which he justly made of paramount importance. Velpeau surpassed him in book-learning, as well as in versatility and generalizing power, but he was inferior in practice, still more inferior as a clinical teacher. So attractive were Nélaton's clinical prelections for their method, their comprehensiveness, their exquisite lucidity, that his audiences, numbering representatives French and foreign, students in their novitiate as well as middle-aged practitioners, were so large that it was difficult to get near enough to him in his morning rounds. He wrote a little; but his treatise on surgical pathology (of which the

first three volumes were exclusively his own, and the last two, written from his materials by Dr Jamain) possesses the qualities of his oral exposition—clearness of classification and expression. What wonder that such a man rose to be the most esteemed surgeon in Europe? Neither Dupuytren nor Velpeau ever realized his income, which in some years was no less than £20,000. So highly did the Emperor value him, that after loading him with every mark of confidence and respect, he departed from traditional usage so far as to make him a senator of the empire—an act as creditable to Napoleon as to Nélaton. Only two other medical men shared with him the rank of Grand Officer of the Legion of Honour—Ricord and Larrey.—*Scotsman*, Sept. 22.

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- Charles E. Buckingham, M.D.,—The Proper Treatment of Children, Medical or Medicinal. Boston, 1873.
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- Ecker on the Convolutions of the Human Brain, translated by John C. Galton. London, 1873.
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- Jabes Hogg,—Parasitic Origin of Skin Diseases. London, 1873.
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- General Report of Lunatic Asylums, Vaccination, and Dispensaries in Bengal Presidency for 1871. Calcutta, 1873.
- Æneas Munro, M.D.,—The Science and Art of Nursing the Sick. Glasgow, 1873.
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- American Journal of the Medical Sciences,—July.
- American Practitioner,—June to October.
- Annales d'Oculistique,—May to August.
- Annali di Chimica applicata alla Medicina,—May to September.
- Archives of Ophthalmology and Otolgy,—Vol. iii., No. 1.
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- Berliner Klin. Wochenschrift,—June 2 to October 20.
- Births, Deaths, and Marriages, Monthly Return of,—May to September; Quarterly Return, March, June; Supplement for 1872.
- British and Foreign Medico-Chirurgical Review,—July, October.
- British Medical Journal,—May 31 to October 25.
- Bulletin Général de Thérapeutique,—May 30 to October 15.
- Canada Medical and Surgical Reporter,—June, July.
- Dublin Journal of Medical Science,—June to October.
- France Médicale,—May 28 to October 22.
- Gazette des Hôpitaux,—May 22 to Oct. 21.
- Gazette Hebdomadaire de Médecine, etc.,—May 30 to October 24.
- Gazette Médicale de Paris,—May 31 to October 18.
- Glasgow Medical Journal,—May, August.
- Indian Medical Gazette,—May to July.
- Journal de Médecine et de Chirurgie,—July to October.
- Journal of Anatomy and Physiology,—June.
- Journal of Mental Science,—July, October.
- Journal of the Scottish Meteorological Society,—January, April, July.
- Klinische Monatsblätter für Augenheilkunde,—April to August.
- London Medical Record,—June 4 to Oct. 22.
- Lyon Médical,—May 25 to Oct. 12.
- Madras Monthly Journal of Medical Science,—May, June.
- Medical Press and Circular,—May 28 to October 25.
- Medical Times and Gazette,—May 31 to October 25.
- Medizinische Jahrbücher,—Jahrgang 1873, Heft 1, 2.
- Monthly Mirror,—August.
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- New York Medical Record,—May 15 to October 1.
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- Philadelphia Medical Times,—May 17 to September 27.
- Practitioner,—June to October.
- Revue des Sciences Médicales,—July.
- Revue de Thérapeutique Médico-Chirurgicale,—June 1 to October 15.
- Sanitarian,—June to August.
- Students' Journal and Hospital Gazette,—May 10 to July 5.
- Tribune Médicale,—June 15.
- Virchow's Archiv,—June 10.
- Western Lancet,—May to July.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Introductory Address delivered at the Opening of the Edinburgh Medical School on 3d November 1873.* By GEORGE W. BALFOUR, M.D.

GENTLEMEN,—Could we recall to earth one of those skin-clad savages, whose graphic remains are still visible in the caves on the shores of our neighbouring firth, whose kitchen-middens and lacustrine dwellings are yet traceable even nearer home, and who 2000 years and more ago got their precarious living by hunting the stag and the boar in those trackless forests which then occupied the hills whereon our fair city now rests, or by catching the fish or the wildfowl of those lochs which now, as beauteous gardens and fair meadows, surround and adorn our dwellings, we could hardly imagine the wonder and awe with which he would survey the changed scene.

But the awe-struck amazement of this ignorant savage at all the wondrous civilization of this nineteenth century, would be as nothing compared with the admiring astonishment with which Hippocrates—the father of our art—could we rouse him from his long slumber, would regard the present state of medicine. A denizen of Greece—in the palmy days of Pericles and Phidias—we must forgive the scornful smile with which he would glance at our paltry architectural imitations, specially at the unfinished counterfeit of that Parthenon which he remembers in all its virgin beauty. Drawing his pallium closer round him as a keen north-easter whistles past, we shall suppose him to find his way into our Infirmary, up the main staircase, and into one of the side-rooms, where he is courteously received by the attending physician and his clinical clerks, who, mindful of Mr Syme's first axiom—never to look surprised at anything—are politely blind to the scanty raiment of their stranger visitor. Warmed by the genial atmosphere around him, feeling himself, as it were, at home amid the sick and their surroundings, conscious of his own personal reputation and of the esteem in which his views and his words were held for many hundred years, the venerable gentleman essays to impart a little clinical instruction in return for the kindness with which he

has been received. Attracted by the appearance of some fluid collected in glass jars which he thinks he recognises, he proceeds to descant upon the prognostics to be derived from the appearance of the urine. He points out that clouds in the urine are favourable in proportion to the lightness of their colour, but that, however light may be that colour, the prognosis becomes more unfavourable if the cloud should rise instead of fall, and absolutely unfavourable—a fatal sign—if it cause a general turbidity of the urine without any sediment; while a sediment, smooth, white, and consistent, indicates freedom from danger and a short illness; but if the urine be occasionally clear, the disease will be protracted. Yellow thin urine indicates an unconcocted disease, and a danger lest the patient may not be able to hold out till the disease become concocted; while a dark-coloured urine is always bad, and the darker the urine the worse the prognosis, especially if it be accompanied by a farinaceous sediment; and if bubbles settle on the urine, the kidneys are affected, and the complaint sure to be protracted.¹ Thus pleasantly discoursing on the results of his experience obtained in his own infirmary—the Asclepion of Cos—and delighted at the ease with which his clear and incisive, if somewhat dogmatic, sentences have flowed from lips silent for so many hundred years, Hippocrates glances complacently round upon his audience, and finds that, while he has been speaking, one of the young men has, by boiling some of the urine in a test-tube, obtained an opaque, milky-looking fluid, and another a copious brick-red deposit. A little aghast at these peculiar and unexpected results, he wisely says nothing; and when suddenly asked as to what he thinks of the comparative merits of picric and nitric acids as tests for albumen, he feigns a little deafness, but the look of helpless imbecility which begins to steal over his face is not lessened when another inquires whether he prefers Moore's, Trommer's, or Boettcher's test for sugar in the urine, or whether he possesses any other less fallacious, and what he thinks of a milk diet as a curative agent. The words "milk diet" recall the fast-fading colour faintly to his cheek, and with one timid glance at the microscope, beneath which one of the clerks has a preparation, the nature of which he is anxious to know, poor Hippocrates passes out into the ward, discoursing pleasantly of the virtues of ass's and of cow's milk, of their hurtfulness in fevers, and of their curative virtues in gout and in phthisis.² The physician, ignoring his visitor's peculiar ideas as to the use of milk in fever, courteously inquires his views as to tubercle, what he thinks of its relation to inflammation generally, and to catarrhal pneumonia in particular; and, without waiting for his answer, shows him a fine specimen of the cracked-pot sound, and then hands him a stethoscope that he may listen to a peculiarly good example of bronchophony. The word has a Greek ring about it, and thinking he may understand it better with the instrument

¹ *Vide Προγνωστικόν and Ἀφορισμοί.*² *Ἀφορισμοί.*

in his hand, Hippocrates takes it, and, uncertain what to do with it, looks first at it and then through it, considers whether he should listen to it as to the moaning of the sea in a shell, or whether the sound is to be produced by blowing through it as a trumpet, and, utterly bewildered by all he has heard and seen, suddenly recollects an important engagement in Thessaly,¹ and with many thanks returns the wonderful but unknown instrument, and departs a sadder if not a wiser man, fully conscious that if called upon to pass a clinical examination of to-day, he—the great clinician of antiquity—would infallibly be found wanting.

And yet it was by the hands of Hippocrates, and in the Asclepion at Cos, that the torch of medical science was first so trimmed as to give a steady and a brilliant light, and it is only by successive trimmings and fresh supplies of oil that it has attained the refulgence it now possesses. We—your teachers—have done and are still doing all in our power to brighten its light, and in handing that torch to you to transmit to succeeding generations, we do so in the sure hope that among you there are at least some few, and the more the better, who love and reverence truth for its own sake, and by whom medical science shall be yet more greatly advanced in grandeur and utility during all those ages yet to come, as “the great world spins for ever down the ringing grooves of change.”

The first lesson that we learn from this little episode of Hippocrates is the very great advance in scientific accuracy which medicine has made since his days; and though that is evident enough even from the broad touches with which I have painted the scene, yet the reality is so much greater and grander, that time would fail me even to sketch cursorily the more prominent points, nor is it needful on the present occasion. The second lesson, and a most important one it is, is, that the most thorough knowledge of ancient medical lore and the most perfect acquaintance with the purest Ionic Greek is of no practical value to the medical student of the present day, and probably it has therefore been placed amongst the optional subjects of the preliminary examination by the Royal Colleges. We are not here to cavil at the course of study laid down for us, but reverently to obey it, and to show how and in what spirit it may be most profitably pursued, otherwise I might have been tempted to ask why Latin should be made compulsory and Greek only optional, and this all the more that the latter is still a living language, while the former has long since been dead. The truth is, however—and it is well to remember it—that there is only one language in the world of any great importance, and that is our own. It is the most widely diffused of any language in the world, and in it are published the results of the scientific labours of the two greatest nations on the globe, while its simple grammar and the almost equal combination of Teutonic and

¹ With the worms, probably; that being his supposed burial-place.

Romaic elements of which it consists make it more easily acquired by most Europeans than any other. For these reasons, and because he believes that English will one day become the common scientific language of the world, the senior Professor of Zoology in the University of Upsala, Dr Thorell, has recently published in English his great work of 644 pages on the Synonyms of European spiders, not a very important work in our eyes, perhaps, but a most important work nevertheless, as showing that in Dr Thorell's opinion—and he is no mean judge—the largest circle of scientific readers is to be reached through the English language. This, therefore, is a strong inducement to us all to cultivate purity in our language, always preferring words of Teutonic to those of Romaic origin where there is any choice, not only because they are always more forcible, but also out of respect to our Teutonic cousins, who rank next to ourselves in number and importance; and to seek by accuracy in our grammar, and by expressing ourselves in a logical manner, to ensure that we not only understand ourselves, but can be understood of others. This is a matter which concerns our everyday usefulness: not a day passes in which we are not called upon to form an opinion as to the nature of a case, and to suggest a treatment; but we cannot do either the one or the other without reasoning consciously or unconsciously, correctly or incorrectly. Surely, therefore, I am justified in pleading for the study of logic, if not as part of your compulsory instruction, at least as a most necessary part of that education to which you ought all voluntarily to submit, that you may learn the nature and rules of evidence, and of the procedure of your own minds in the search after truth. Arithmetic, algebra, and geometry, which are all compulsory subjects of the preliminary examination in general education, owe that position mainly to the mental training they afford; but this is far inferior to that to be obtained from the study of logic, being of a much more elementary character.

But we are not quite done with Hippocrates yet; and the third and most important lesson which I would have you learn from him is what you ought to think of the profession you have adopted, and in what spirit you ought to pursue the studies which are to prepare you for its exercise. In his work *περι τεχνης*,¹ Hippocrates says, "What I understand by the science of medicine, is to deliver the sick completely from their sufferings, to mitigate the intensity of disease, and to undertake nothing for those overcome by their malady." With this latter sentence, however, no right-thinking physician can have any sympathy; to preserve, and, if possible, to prolong life, is the great object of the medical art; but every physician knows that hope is the best of all restoratives, while anxiety and fear act as deadly poisons, and that the life of a patient may be as effectually shortened by the words and manner

¹ Of course I am aware that this work is believed not to have been written by the great Hippocrates, but as it undoubtedly emanates from the school of . . . it no doubt conveys his views.

f the physician as by any overt act on his part. We must never forget, then, that even the inevitable and recognised approach of

"That Shadow cloaked from head to foot,
Which keeps the keys of all the creeds,"

Does not absolve us from the discharge of our final duty to our patient—to promote his euthanasia. Even in the stage of dying, the physician ought not to forsake the sick; even then he may become a benefactor, and, if he cannot save, may at least relieve departing life. But with this exception, we may accept the definition of medicine just given as correct. It is the art of relieving suffering and curing the sick, and all your studies ought to be carried out with this end ever in view.

Too many regard the examination for the diploma at the end of their studies as all that has to be prepared for; yet that may be passed, and the new-fledged licentiate be very ill prepared for the duties about to devolve on him. On the other hand, no one can diligently pursue his studies with the great object of the medical art ever before him, without finding himself at the end of his probationary period not only fitted for the exercise of that art, but also, and therefore, qualified to pass his examination. And keeping this end constantly in view, it is astonishing what a new charm is lent to every branch of study; how interesting and important even trifling matters appear, while the commonest objects seem invested and consecrated with a halo of science. Nor am I merely using sounding words without significance: take, for example, the optional subject of Natural Philosophy—which none of you should, however, omit attending a course of—and see with what interest the dry details of hydraulics, of mechanics, and of pneumatics, become invested, when we come to understand that the same great laws which regulate the movements of the solids and of the fluids around us are equally applicable to explain the movements of those of which we consist. This identity of those laws, by virtue of which we live and move with those which regulate external nature, opens up wide views of the cosmogony of which we are a part, the grandeur of which is not lessened when we reflect that the same laws which produce the soft breezy murmur of natural respiration also give rise to the howling of the wintry storm; and that the lightning's brilliant flash is but the vivid manifestation of a force which pervades all nature, and which is so ever present in every act evincing life, and so intimately connected with our own organism, that by many it has actually been supposed to be life itself, though indeed it can lay claim to no higher title than the *anima mundi*—if you will, the *anima hominis* is of a different stamp.

Again, if we take the subject of Natural History, which is recommended by the Colleges, and which I cannot see that you can well do without, and which includes both Botany and Zoology, though these are usually taught separately, a very cursory inquiry will

show how important an influence some of the commonest objects are exercising, or may be made to exercise, on both medicine and science. Between Botany and Zoology there is a vast borderland occupied as some believe by spores or germs, some of which develop into fungi, which are indubitably plants, while others develop into Bacteria, which are at least apparently animals (and so they were regarded by Ehrenberg); while others look upon all these bodies—Fungi, Bacteria, etc.—as Protista, or members of a primordial kingdom, which develop under certain chemical agencies without pre-existing germs, and from which both plants and animals may originate by gradual modification of their simple organisms. In this borderland there is now being fought out the Battle of Life, as we understand these words, the great question of creation or development. By investigating this obscure corner of His kingdom, our finite minds are endeavouring to ascertain whether the Infinite Mind created individuals with power to propagate their kind, or merely matter organizable under certain conditions and in certain circumstances. Nor must we regard this question as one purely scientific, and of no practical value. In this city we need use no vague expressions, we can state exactly how much this question is worth to us, for the Managers of our Infirmary have deliberately stated that a belief in the panspermic hypothesis and in its congener, Pasteur's vital theory of fermentation, is worth six hundred pounds a year; a most extraordinary sum to stake upon a mere hypothesis, when we poor physicians are expected to provide microscopes and thermometers for ourselves, articles which are of truly practical value, and which would not cost a tithe of the money. In making this statement, I by no means wish to invalidate Mr Lister's great success in treatment,—that I have no desire to rate otherwise than as he himself puts it; but as it is evident that under almost any other theory similar results might be attained for a merely nominal sum, I am justified in stating that the panspermic hypothesis costs our poor Infirmary no less than six hundred pounds a year.¹ Moreover, the panspermic and development theories have reciprocally important bearings on the cure and on the prevention of those many troublesome diseases which depend upon the presence of microscopic plants upon or within the organism; and, passing thus into the more truly botanical field, we find that a great many of those important phenomena which we are accustomed complacently to term animal, can be most conveniently studied in members of the vegetable kingdom. For long, Botany was a mere collecting, naming, and classifying of plants; even yet it has scarcely got beyond the stage of organography; but a bright future awaits it when once it fairly gets into the domain of practical

¹ The past history of surgery, coupled with the present attitude of scientists towards Pasteur's theory, fully justify this statement. I respect Mr Lister almost as much as I venerate Hippocrates; but in these days progress is more rapid, and though a friend of Plato, I am a greater friend of truth.

physiology. One of our number, Dr Pettigrew, has already sought to make his mark by connecting the phenomena of vegetable with those of animal circulation; but the field of inquiry is not restricted to one department, it is commensurate with the domain of life. An explanation of the cause of the movements of the Bacteria, the Desmidiæ, and of the Diatomaceæ, cannot fail to have an important influence in regard to our knowledge of the movements of animal life; while an investigation into the mode of production of the movements of the Mimosa, the Hedysarum, and the Dionæa, which seem to be of a different character, cannot but afford information as to the mode in which a stimulus is perceived, this perception constituting the essence of a nervous system. Even conscious intelligence seems to be possessed by plants. Under this head I scarcely rank those remarkable phenomena whereby the flowers we tread on in our daily walks seem to know much better than we do what-o'-clock it is, which way the wind blows, and what weather we may expect; because, however wonderful these phenomena may seem when thus put into words, they may all ultimately be referred to the perception of an external stimulus; and though that can hardly be regarded as a more purely material act than the perception of an internal stimulus, which volition essentially is, yet there are other phenomena existent in the vegetable world which seem less equivocally the result of the possession of conscious intelligence, and which cannot apparently be referred to anything else. Thus, if a rose-bush be planted with dry earth on one side and rich soil on the other, the roots at first start out all round as usual, but those that begin their journey through the poor dust speedily receive mysterious warning of the better land lying in the opposite direction, and they all turn back to follow their companions who have gone into the rich pasturage. Again, various examples are on record of trees which, growing in barren situations, as on the top of a wall, have thrown all their vigour into one rootlet, which was sent downwards to seek for better soil whereby it might be nourished; and in Silliman's *American Journal of Science* for 1840 we read of an elm which, growing on the top of a boulder, became accidentally detached on one side, so that its trunk lay parallel with the earth. As if conscious of its uncomfortable position, it sent out a branch from one of the most vigorous of the detached roots, which, taking a hitch round a projecting angle of rock, again united with the parent stalk, and this, thus supported, in a few years regained its vertical position, and when last seen was a tree fifty feet high and four and a half feet in circumference. But, indeed, though those instances seem conclusively to point to the possession by plants of what we cannot otherwise define than as a conscious intelligence, limited certainly in its external manifestations by the absence of organs and occasions, and possibly limited in its very essence, they are not more wonderful than the motions daily exhibited, by which the radicle constantly seeks the ground, while the plumula shoots into

the air; or those by which some of the higher phanerogamous plants twist in spirals round objects near them, and by which all present one side of their leaves towards the light—motions which can neither be regarded as accidental or merely mechanical acts. While as if to add to our wonder, we find that in quite recent times it has been shown that certain electrical phenomena, which accompany the contraction of muscle and the activity of nerves in animals, also occur in those parts of plants which exhibit similar functions; and Mr Darwin is said to have made observations which tend to prove that the leaves of the *Dionæa* not only catch flies but also digest them.¹ All these facts tend to connect the vegetable kingdom very closely indeed with the animal one, and to make the study of vegetable physiology a most important means of obtaining information; while from the many great and evident advantages it possesses over animal physiology in facility of research, it is very likely to spread the study of practical physiology over a much wider area, and may thus possibly hasten the attainment of important results.

Thus you see what an intensity of scientific interest gathers round the careful observation of such very common objects as a little green mould, the leaf of a plant, or drop of putrid fluid.

Nor is it otherwise if we turn to the animal world. I shall give but one example. You are all aware of the great excitement produced in the scientific world by Darwin's announcement of his ideal law of development; yet one of the commonest objects about us is a frog, which begins its free existence with the organization of a fish, and, after undergoing a remarkable metamorphosis, attains the condition of an air-breathing quadruped, capable of easy and rapid terrestrial locomotion, and drowns in that fluid in which it formerly lived. Now, if there be any truth in the statement that all the various species of animals have arisen through ordinary generation, one from another, by a process of development, then the life-history of so singular an animal as the frog may with reason be expected to have some bearing upon such a process. About this very common object, therefore, are gathered biological questions which bear upon the origin of species, upon the course and mode of organic development, as well as upon other speculative problems to which answers are as yet far to seek.²

We see, then, that these optional subjects—Natural Philosophy and Natural History—contain not only a vast amount of most interesting information, without a competent knowledge of which no medical practitioner can have any pretension to call himself well

¹ *Vide* paper by Dr Burdon-Sanderson on the Electric Phenomena which accompany the Contraction of the Leaf of Venus's Fly-trap (*Dionæa muscipula*), read at the last meeting of the British Association for the Advancement of Science, September 1873.

² *Vide* paper on the Common Frog, by St George Mivart, in "Nature," for 2d October 1873.

informed, but also convey a great deal of instruction, without which it is impossible to recognise many diseases, and which has a most important influence both upon the treatment and upon the prevention of others. And the only reason, I conceive, why these subjects are not so popular or so universally studied as they well deserve to be, seems really because they are so utterly neglected during early life, when the mind is usually so open to material impressions, and so little able to reason or to understand even the simplest process of ratiocination. Yet this is the period selected for drumming into the youthful mind all the scholastic intricacies of grammar; while, if the natural sciences are taught at all, all their delights are veiled beneath dry systematic details, which are too often regarded as their needful foundation, instead of merely as an important culmination, which they really are. Yet the simple facts of natural science present details which are no less curious and attractive to the eye of childhood, than they are important to the philosopher in their bearing upon the general laws of nature; for a philosopher is but a child of larger growth and wider views.

But to turn now to the more purely professional part of your studies, I would have you remember that you must study each branch with diligent care, and with an eye not to future examinations—that makes it mere drudgery—but to its future usefulness as a means of aiding you in the cure and prevention of disease; and in this connexion it is astonishing how much interest centres round even the driest minutiae. You must, therefore, in studying Anatomy, remember that you are not busy with an abstract science, but with the physical basis of medicine. In regard to surgical diagnosis, the external configuration of the body must be carefully conned, and compared with the bones, the joints, and the outlets of the skeleton, so that we may familiarize ourselves with the peculiar feeling of the normal state of all these parts through the integuments, and thus the more readily detect any deviation; while a knowledge of the position and insertion of the ligaments and muscles surrounding a joint may frequently afford us the greatest aid in reducing a dislocation and in determining the amount of injury the joint has received; at the same time that the origin and insertion of the muscles of a limb, and their consequent line of action, guide us to the right position to place that limb when fractured. It is self-evident, also, that a knowledge of the position of all the large arteries, in regard to certain fixed points, as well as their relations to their concomitant veins and neighbouring nerves, is of the very utmost importance, not only for the safe carrying out of any operative interference upon them, but occasionally even for the mere preservation of life in the case of sudden accidental hæmorrhage. In short, while to be a successful surgeon it is necessary to be an accomplished anatomist, the merest tyro may occasionally be the fortunate means of saving life merely by his anatomical knowledge and his presence of mind in

applying it. Passing by, for the present, the many other important applications of anatomical proficiency to surgical practice, we may ask what possible connexion anatomy can have with medicine; here we have only symptoms to treat, not dislocations to reduce. But without a very competent knowledge of anatomy, how could we decide as to the origin of an abdominal tumour, and thus determine its treatment? how could we be led from subscapular pain to suspect the existence of liver disease? how could we know which is the base and which the apex of the heart, and thus clearly understand the course of the circulation, and the nature of its derangements? or how could we understand the direction in which the lungs would be compressed in pleuritic effusion, and thus determine the best position for tapping, or give a rational answer to one of the many similar problems which come daily before physicians, and upon a correct solution of which depends the diagnosis of the disease, and therefore always the relief and often the cure of the patient? Instead of being of less importance, a thorough knowledge of anatomy is of even more consequence to the physician than to the surgeon, though, because the actual source of medical ailments is not usually so obvious as that of surgical ones generally is, he requires to ally his anatomy with its sister science Physiology before he can track the lesion to its origin. For, as anatomy teaches us the structure of the body, so physiology teaches us the functions of each separate part of that structure; and as surgery occupies herself most with lesions of structure, so lesions of function come most frequently under the care of medicine; while effectually to relieve, and certainly to cure, or even to give a reliable prognosis, it is necessary that we be able to trace the lesion of function to its anatomical source. How otherwise could we, with any hope of success, propose to treat obstinate neuralgia of the shoulder-joint or amaurotic blindness by a similar procedure—the extraction of a carious tooth? yet in each instance this has been done. A thorough knowledge of the functions of the various parts of the cerebro-spinal system, and of the anatomical direction of their fibres, enables us nowadays readily to separate one from the other many diseases of the nervous system which were formerly slumped together, to assign them their local habitation, to give in each case a tolerably definite prognosis, and, as you can readily understand, to treat them with a much greater prospect of success. To do this profitably, however, we have first to apply for needful information to Pathological or Morbid Anatomy; and confining our attention at present to the spinal cord, we find that morbid anatomy teaches us that one predominant lesion of function—paralysis—may be associated with two very different conditions of the cord; in the one case we have an excess of blood and a congested condition of the bloodvessels, while in the other we have the very reverse—a pale exsanguined condition of the parts affected. Both may occasionally go on to softening, but in each case this softening presents a different appear-

nce, and depends upon a different cause; and while morbid anatomy exhibits the parts and describes their appearance, it is from Pathology itself that we obtain a rational and connected history of the various modes of causation of these lesions and of their possible terminations. From Systematic Medicine we get a succinct history of the various symptoms common to those two forms of lesion as well as of those in which they differ, and which constitute their differential diagnosis; and from it we also acquire a knowledge of the various methods which have been adopted to cure that disease or disordered manifestation of life depending upon each peculiar lesion, or to relieve the sufferings which result from its interference with the normal discharge of function. While Clinical Medicine exhibits to us an actual example of the disease in question, points out the various symptoms and signs by which it may be recognised and differentiated from other diseases, passes in cursory review the various methods of treatment which have been propounded, and finally selects that which, for reasons given, seems most appropriate, and proceeds to apply it. But the selection of the remedy proceeds not on any merely haphazard fashion, but upon information already received from the teachings of what we term *Materia Medica* and *Therapeutics*, which instruct us in a two-fold fashion—first by pointing out what have been the results already obtained by the employment of certain remedies in certain diseases; and, second, by showing what are the actual physiological effects of certain remedies, and then suggesting their employment in those diseases in which the production of these effects seems desirable. For instance, in reference to the two forms of disease of the spinal cord, both productive of paralysis, we are taught that our forefathers employed various remedies in such cases with very various results, and without any very definite idea as to what they were doing, except that they were employing in a case of paralysis, whose cause was unknown, a remedy which some one had found useful in another case of paralysis whose cause was also unknown, the mode of action of the remedy being even less known. Of course it would have been contrary to reason to expect that remedies so applied could have been successful except in the most accidental manner; the consequence was that no certainty was felt, and so-called remedies were apt to be changed with the same thoughtless rapidity as a kaleidoscopic pattern. Now the case is quite different. I have already told you that we know that paralysis may depend upon two different conditions of the cord, which we are able to recognise; but we also know from the researches of modern science that there are two sets of remedies, with distinct actions, one of which increases while the other diminishes the amount of blood in the cord; and by the judicious application of the appropriate remedy to the proper case, we are able to attain with comparative certainty results which our forefathers could not aspire to.

All the interests of professional life centre round the relief of

suffering and cure of disease—that is, round practical medicine and practical surgery also, of course; all that I have said referring equally to surgery, only the illustrations would require to be changed; and yet we see how the successful practice of medicine is dependent upon a thorough knowledge of *Materia Medica*, *Pathology*, *Physiology*, and *Anatomy*, how impossible it would be to diagnose, to prognose, or to treat disease properly without an accurate acquaintance with these subjects. You will remember how, in times long past, the Dogmatists proclaimed the necessity of founding medical practice upon such a knowledge of those sciences as was then to be obtained; while the Empirics decried them as such, and boldly asserted that experience was all that was requisite to make a man a physician or a surgeon. But time has changed all that, and by substituting precision in our information in regard to these matters for mere vague guesses, has at once widened the base of our experience and made it more accurate, so that the enlightened empiricism of our day rests securely on the broad foundation which the Dogmatists commenced to lay; and we have relegated to an earlier stage, and to more truly preliminary studies, the statement of the moderate and Methodic Celsus, that, though these things do not make a man a physician, they at least fit and prepare him for becoming one.¹

You will observe that I have omitted all notice of those ordinary subjects of professional education, *Chemistry*, *Midwifery*, and *Medical Jurisprudence*; but two of these are as much outcrops of general medical study as medicine and surgery themselves, while the third stands a good deal by itself. A former distinguished lecturer in this school stated that upon one occasion, while teaching his students the properties of copper, he was interrupted by a very clever physician demanding why he thus wasted the time of his pupils when he ought rather to be teaching them to analyze tubercle. "And yet," added the chemist, "had my demonstration been on the very matter this doctor demanded, I must have begun with teaching that tubercle could not be analyzed without the use of the oxide of copper."² I myself would go a step further, and say that I do not see that we have any business to waste our time analyzing tubercle at all; we must leave that to the chemists. Chemistry, and especially organic chemistry, is so vast a subject, and requires in any experimental investigation so much accuracy in details, and so complete a knowledge of possible fallacies, and of their source and mode of correction, that it is utterly hopeless for any one but a professed chemist to carry them out successfully. Without a moderate knowledge of chemistry, however, it is impossible to understand the aeration of the blood, the digestion of the food, etc., or to detect various morbid products, such as sugar or albumen in the

¹ Milligan's "Celsus." Edinburgh, 1826. *Liber primus*, p. 11.

² Vide Dr George Wilson's Introductory Address, "Edinburgh Medical Journal," 1850, p. 561.

urine, therefore chemistry is very properly included among the subjects of professional education ; but we must never forget that all we get is a mere—though a very necessary—smattering, and that on all important occasions we must apply to a professed chemist, while medicine will assuredly halt until we have a practical chemist attached to all our large infirmaries. Midwifery, however, is as independent an art as medicine or surgery, and rests like them upon the same foundation of natural science—the most ordinary case is a most interesting problem, involving a thorough knowledge of anatomy, physiology, and mechanics ; while, if the slightest halt take place, the *materia medica* may require to be called upon, or it may even be a question whether surgical interference may not be required ; we cannot therefore practise midwifery successfully without a perfect acquaintance with all the ordinary branches of medical study. The subject of Medical Jurisprudence is in precisely the same position ; it is based upon a thorough knowledge of every department of medicine, and to this must be added a knowledge of forensic procedure which is best obtained by practical instruction, for which there is ample opportunity in this School. Besides this, the recent Public Health Act, which enacts the appointment of medical officers of health by every urban and rural sanitary authority throughout England and Wales, must of necessity elevate into still greater importance, if this be possible, all the subordinate branches of medical study ; for the practitioner holding such an appointment will very soon be taught that he has no longer to deal with a blindly credulous public, but with men trained to detect sophistries, who will not accept a smattering of law as an excuse for an absence of precision in medical knowledge.

There are also many special studies, such as Ophthalmic and Aural Surgery, and Medical Psychology, all of the greatest importance for the well-being of mankind, but all merely outcomes—finials, as it were, of the great structure of general medical education. I strongly recommend them all to your careful attention, for without some knowledge of them no medical man can regard himself as thoroughly qualified, as, even though not specially qualified himself, he must frequently be called upon to decide whether a specialist should be called in or not.

In all your studies, however, I must recommend you to remember well the old saying, *Non multa, sed multum*, you must not divide your attention over too many subjects at once, otherwise you speedily lose heart at the vastness of the field of study, but must master one before you proceed to another. Sidney Smith used to say that there was nothing like taking “short views” for enabling one to get through life comfortably ; and the same thing is true of medical study. If you really could, however vaguely, realize the amount of information you will be required to obtain during the next four years, most minds would, I think, shrink appalled from the task ; and yet, thanks to short views and one step at a time, if you are

conscientious in making your foothold secure before attempting to advance, you will all in due time reach your desired goal. But in everything you must remember *Non multa, sed multum*, a rapid transition from subject to subject is apt to impair the faculty of sustained attention, and to induce a superficial, uninquiring, and far from independent habit of mind. You must ever remember that experience, which it is all our desire to obtain, is not the result of observation merely, but of reflection. A most distinguished clinical author of last century says, "There are only fifteen beds in the public hospital of this place, and I do not wish for more. . . . It is not eating alone, but digestion, that gives strength. A physician who tells us that he visits a hundred and fifty, or even a greater number, of sick people daily, has, in my opinion, so little pretensions to the title of an experienced practitioner, that I would even deny he had any experience at all. For nature is not so complaisant that she will unveil herself at once to whoever casts eyes on her."¹ You must, from the very first, frequent the Infirmary, and accustom yourself to the sight of disease and suffering, without at first attempting to understand what it is all about. Much that you thus see, without comprehending, will hereafter recur to your memory and find its fitting explanation; and knowledge thus obtained is never forgotten, and you will, day by day, pick up information which will open up the ultimate intentions of your studies and render even their driest detail a matter of interest. And even as you feel yourselves to advance necessarily step by step, a knowledge of medical history will teach you that medicine itself has so advanced, and is still advancing. Day by day the fundamental sciences upon which our art is based are giving us newer and more accurate information as to our functions, their mode of operation, and reciprocal action one upon the other; while, day by day also, the wonderful science of chemistry is giving us new weapons wherewith we may combat disease and death. We are always moving onwards, the medicine of to-day is not that of yesterday, and to-morrow will find us still further advanced. I would that time would permit me to recount the great progress that medicine has made in the short period that has elapsed since I myself sat a tyro on these benches: alas! a day would not suffice to make that progress intelligible in all its relations to the past, and all its bright hopes for the future. But a cursory reference to a single disease may suffice to indicate my meaning. I shall take as an illustration the treatment of delirium tremens. Forty years ago delirium tremens was regarded as caused by a violent excitement of the cerebrum, which had to be reduced by the most powerful sedatives to prevent death, the axiom being that the patient must sleep or die. Under the influence of this idea, so long as blood-letting was regarded as the great sedative, it was used freely in all such cases. By-and-by, as the importance of sleep and

"Medical and Surgical Observations." By A. G. Richter, M.D. Translated from the German. Edinburgh, 1794. Preface, p. 15.

the danger of bleeding became recognised, very full doses of opium were employed, which were scarcely less dangerous than the bleedings they were employed to replace.¹ Shortly after this an American physician, Dr Ware, pointed out that delirium tremens was a disease which had a tendency to terminate naturally in sleep at the end of sixty or seventy hours, provided only the strength was supported by nutriment;² and this was followed by the free use of padded cells, in which the patients, though preserved from the risks of physic, were exposed to various other risks from constitutional exhaustion and exposure to cold, etc.; and with this phase of treatment was associated the dogmatic enunciation that "the doctrine of the definite course and duration of delirium tremens ought to make us slow to entertain any extravagant expectation of materially abbreviating its duration by therapeutic measures."³ While another authority declared that "food is *the* stimulus, *par excellence*, for the brain which frequent narcotism has reduced to the state in which delirium occurs, . . . in all these cases the action of food may be supplemented or partially replaced by stimulant doses of alcohol, ammonia, etc., but *true narcotics are injurious*."⁴ Unfortunately for the truth of both of these statements, chemistry just at that time supplied us with a *true narcotic* which not only does no harm, but which actually cuts short within twelve hours what it took nature from sixty to seventy to accomplish. From all this we learn that most important lesson, that our power to cut short disease is limited, not so much by the processes involved, as by our ignorance of the means to be employed, and that a brighter future for therapeutics may be looked for, once we have shaken ourselves free from the idea that the processes of nature cannot be successfully interfered with by art.

And now, Gentlemen, time warns me that I must conclude. You are just entering on your preparation for "the most sublime vocation of man;" for next after the service of the Deity, and, indeed, scarcely second even to that, is to be a priest of the holy vital flame, and an administrator of God's highest gifts, and of the most secret powers of nature—in one word, a *physician*. And you must ever remember that when you at length appear before the throne of Eternal Truth you will be asked this question: I made you steward of the wonderful powers which I have placed in nature and her products to benefit mankind; how did you distribute those treasures? for the benefit of mankind, with gratitude and adoration? or for the honour of your own name, with selfishness and egotism? Did you, in your researches and actions, strive merely for truth, to help your brethren, or was it all for self-interest? To him to whom

¹ Dr Gairdner's "Clinical Medicine." Edinburgh, 1862. P. 262.

² "Transactions of the Massachusetts Medical Society," 1831.

³ Paper by Dr Cuming of Belfast in the "Dublin Quarterly Journal of Medical Science," for February, 1870, p. 74.

⁴ Anstie "On Stimulants and Narcotics." London, 1864. P. 139.

medicine becomes not a religion, it is the most disconsolate, troublesome, and ungrateful art on earth; yea, it must become with him the greatest frivolity and sin: for only that which is done in God, is holy and beatifying."¹ Let us, therefore, always remember that *laborare est orare*, that we can only show our faith by our works, and let us strive so that we may, each of us, apply to ourselves and to our daily lives, those noble words spoken by Dr Holmes, with allegorical reference to the structure of the Chambered Nautilus:—

“Build thee more stately mansions, O my Soul,
As the swift seasons roll!
Leave thy low vaulted past;
Let each new temple, nobler than the last,
Build thee to heaven with a dome more vast,
Till thou at length art free,
Leaving thine outgrown shell by life's unresting sea.”

And now I have briefly to refer to the changes which have taken place in the teachers of this School. Since we last met here Dr Arthur Gamgee has received the reward of his great abilities and scientific acquirements, by being appointed Professor of Practical Physiology in the Owen's School of Medicine, Manchester. This, I am glad to say, has been our only loss, if loss we can call it; for, though it has been an infinite gain to Manchester, yet it can scarcely be called a loss to us, since it has been the means of bringing amongst us a man like Dr Pettigrew, whose researches into the anatomy of the heart, the mechanics of flight and of animal movement generally, and the physiology of the circulation, have already gained for him a European name, which will no doubt be increased by the lectures on Physiology which he now commences. Dr Granger Stewart, who is well known to you all, commences this year a course of Systematic Medicine. And we have, on the most important subject of Insanity and Psychological Medicine, two new names to enrol—Dr Clouston, the successor in Morningside Asylum of our old and much-esteemed friend Dr Skae, whose inquiries have extended mainly in the direction of the therapeutics of insanity; and Dr Batty Tuke, whose name is well known as a most able investigator into the physical causes of insanity; both of these gentlemen we welcome as most important accessions, and I trust they may both receive an encouragement proportionate to their deserts.

¹ Hueland's "Enchiridion Medicum."

ARTICLE II.—*On Relapsing Fever; with Special Relation to the Epidemic in Bradford in 1869-70.* By A. RABAGLIATI, M.A., M.D., Honorary Medical Officer to the Bradford Fever Hospital.

Definition.—A contagious disease, which is chiefly met with in times of scarcity or famine. Symptoms:—An abrupt or gradual invasion, marked by rigors or chilliness, quick, full, or bounding pulse, white moist tongue, sometimes becoming dry and brownish; tenderness at the epigastrium, vomiting, and often jaundice; enlarged liver and spleen; constipation as a rule; high-coloured urine; severe headache, felt chiefly in the vertex and back part of the head; dry and hot skin; no characteristic eruption, though one occasionally appears; pains in the back and limbs; restlessness and occasionally acute delirium; an abrupt cessation of all these symptoms, with free perspiration, from the fifth to the ninth day. After a complete apyretic interval, during which the patient may get up and walk about, an abrupt relapse on or about the fourteenth day from the first commencement, running a similar course to the first attack, and terminating on or about the third day of the relapse; sometimes a second or even a third relapse; mortality small, but occasionally death from sudden syncope, or from suppression of urine or coma; after death no specific lesion, but usually enlargement of the liver and spleen.

The epidemic of relapsing fever which appeared in Bradford in 1869-70 might be fairly described by the above definition, which differs in four rather important particulars from that of Dr Murchison. These are:—1st, The invasion. Dr Murchison says it is abrupt. My experience is that it is very often gradual, preceded by a period of malaise, though I have seen cases where onset was as sudden as that of typhus or pneumonia. 2d, The condition of the bowels, which Dr Murchison describes as constipated. Constipation was not a marked feature in the Bradford epidemic. 3d, The position of the headache. The reference by the patient of the headache to the back part and vertex was one of the leading symptoms which enabled me to distinguish relapsing fever from typhus, even at an early stage. 4th, In the very important matter of the rash, I confidently assert that, while there was no rash present in the majority of our cases, there certainly was one in a minority of them. In this particular the Bradford epidemic appears to have differed from all previously described epidemics of relapsing fever in Britain.

I do not make much of Dr Murchison's statement in the definition that the relapse occurs on the fourteenth day, since he says in the text that it takes place from the twelfth to the twentieth day, which coincides pretty well with my own experience.

History.—On this point, as on others that will be referred to in the course of this paper, I do not propose to give a mere résumé of

what may be found more fully stated elsewhere, but rather to dwell on points not much referred to by others.

An observation has been made, originally, so far as I can find, by Dr Spittal of Edinburgh, and referred to by Murchison, Begbie, and others, that an epidemic of relapsing fever was described by Hippocrates as having occurred in Thasus, off the coast of Thrace. I have not been able to find Dr Spittal's original observation, and therefore do not know in what terms he may have referred to this subject; but no one who has read the passages in Hippocrates can have any doubt of the truth of Dr Spittal's statement. Curiously enough, Hippocrates seems to mix up with his account of relapsing fever other cases, whose connexion with it it is difficult to discover. For instance, it seems very doubtful whether a patient who, like Heracleides,¹ had crisis on the twentieth day, and in whose case no mention whatever is made of relapse, can be considered to have had relapsing fever at all; even although epistaxis, enuresis, and diarrhoea are mentioned as phenomena observed at the crisis, and jaundice as a phenomenon observed in the course of the disease. Other cases, again, which are cited, must have followed an anomalous course, if they are cases of the disease at all. Thus Antiphon,² we are told, had discharges (seemingly from the bladder, for the author has just been talking of the urine) which became bloody on the twenty-fourth day, and he had a complete crisis on the fortieth day. This is about twice the ordinary duration of the disease. In another case "tumours³ arose beside the ears and disappeared again, and a weight was felt in the hollow of the left side and above the top of the ischium." With a little stretch of imagination it may be possible to convert these symptoms into a description of the bloody discharges from the ears sometimes seen in later days in aggravated cases, and the enlargement of the spleen so commonly observed; but what are we to make of the first accounts referred to? Or of statements such as this? "If the initial fever lasts one day, the intermission lasts one day also,"⁴ where Hippocrates seems to describe intermittent fever. Still, after making all allowance for anomalies in the account of Hippocrates, it must be admitted that the mass of his cases look uncommonly like cases of relapsing fever, and, indeed, that it is impossible they could have been anything else. This epidemic, like those seen in our own time, does not seem to have run a perfectly definite course, though the account would lead us to believe that in most cases the disorder had run itself out in seventeen days. Thus, in two brothers who were attacked in the very same hour, the phenomena were these:—In one the initial fever lasted seven days, in the other six; the intermission in one five days, in the other six; and the relapse in both five days, so that they recovered together.⁵ In other cases the initial fever, intermission, and relapse lasted, respectively, five,

¹ De Morbis Vulgaribus, i. 2, sec. 3 F, p. 951.

² Ibid., p. 953.

³ Ibid., i. 2, sec. 3 A, p. 953.

⁴ Ibid., p. 956-7.

⁵ Ibid., p. 956.

seven, and five days; seven, seven, and three days; six, six, and five days; and again, six, seven, and four days. It will be seen that the addition of the three numbers in most cases gives a total of seventeen days, which corresponds very well with the length of the disease as we now see it. Besides a large number of curious prognostics to which the physician of Cos directs attention, he dwells on four important phenomena, whose occurrence at the period of crisis might be looked on, separately or together, as the almost invariable precursors of a favourable issue. These are, epistaxis, enuresis, diarrhoea, and dysentery. Sweating he also considers a favourable sign. That is, translated into the medical language of the present day, when secretion and excretion were free and abundant, the case was favourable; when otherwise, the reverse. The symptoms are very characteristic—the splenic enlargement, the jaundice, the tendency of pregnant women to abort, are all mentioned, as well as the frequent appearance of menstruation during the disorder; the two latter symptoms having perhaps some connexion with one another. The first mention of the relapse is in these words:—"And about Arcturus¹ many had the crisis on the eleventh day, and such had relapse without apparent cause." Some of the severe cases (many of which seem to have been fatal), appear, from the mention of the swellings, to have partaken of the character of plague. When suppuration occurred, such cases were always fatal.

The observing powers of Hippocrates are brought into strong relief by the fact that there does not seem to be any other mention of relapsing fever in old writers. It is scarcely to be credited that the disease disappeared till Rutty wrote about it in 1739 as a new disorder. Yet there is no intermediate reference to it. After this, Clark drew attention to it in 1777, and Barker and Cheyne in 1801. Passing over these accounts, and those of the Irish epidemics in 1819, 1826, and 1828, we come to the famous outbreak that occurred in Scotland in 1843. In 1826 the distinction between typhus and relapsing fever had been observed, and in this epidemic in 1843 was further insisted on, particularly by the late Dr Henderson. It was found that the two diseases advanced together, the relapsing fever cases being much more numerous in the beginning of the epidemic; while the typhus fever cases, with their greatly increased mortality, were much more numerous towards its close. Thus, in Glasgow Infirmary,² 2871 cases of relapsing fever were admitted in 1843, and only 142 typhus; in 1844, 432 relapsing fever and 711 typhus; and in 1845, only 37 relapsing fever and 266 typhus; while the mortality is in strict accordance with what we might expect from these facts. The records of cases appended show some instances in which patients who had been attacked here by relapsing fever fell ill of typhus shortly after. It is scarcely to be believed that the same consecution did not occur in Glasgow

¹ The beginning of September.

² Murchison.

and Edinburgh, especially as it was not, at first at least, considered necessary to isolate the cases strictly from one another.¹ It is much to be wished that accurate observations had been made on such cases, as a sufficient number of them would set at rest an idea which observation of the Bradford epidemic has given me, that typhus following relapsing fever is a much less serious and much less fatal disease than typhus occurring alone. The accompanying charts show three or four such cases, where persons who had a typical attack of relapsing fever went through a typical course of typhus shortly afterwards. Now, although the number is very limited, it is rather curious that not only did all these patients recover, but it was further observed that they did not suffer so severely from the typhus as is generally the case in that disease. It seemed as if the previous attack of relapsing fever modified and ameliorated the typhus, giving the sick a better chance of recovery. I think I have heard Dr Sanders express the same idea when speaking of the succession of typhus on typhoid fever, in the same patient, at a short interval of time. Contrary to what one would expect, of the succession of a second specific disorder on another in the person of a patient already exhausted by the first, such patients do not, as a rule, sink in the second fever. If this is so, the curious and interesting question occurs, whether it is possible that we have here the same principle at work as we are already familiar with in the prevention or amelioration of smallpox by vaccination? And may it be that vaccination is itself only a particular case of a general law, which may be expressed somewhat in these terms?

"When the organism has already suffered from a specific disease, it is to some extent modified by it, so that it is less likely to be seriously endangered by another, and generally graver, specific disease succeeding the former, than if the more deadly attacked first."

According to the law, of which this formula may be looked on as the tentative expression, the organism is modified by the pre-existence of a less fatal specific disease, vaccinia, so as to be able with greater facility to throw off the effects of the more fatal specific disease, variola. From this point of view it seems possible to explain even the small mortality of relapsing fever itself, as it may be said that the primary fever (of five, seven, or nine days' duration) inures the constitution to the existence of the febrile state, so that when the relapse occurs after the intermission, the effects are readily thrown off, and the patient seldom sinks. Whether the fact pointed out by Dr Murchison, and above referred to, that typhus and re-

¹ Dr Murchison, quoting Dr Cormack, says that nineteen cases were noted in which patients went through unequivocal attacks of relapsing fever and typhus. I presume this means that the patients all recovered. If so, this would go far to demonstrate the truth of the law laid down in the sequel, that the previous attack of relapsing fever diminished the malignity and reduced the mortality of succeeding typhus.

lapsing fever stand to one another during epidemics of the latter in an inverse ratio, the one disease diminishing as the other increases, and *vice versa*, has any bearing on the existence of a law such as this or not, the fact is at least curious. But there are numerous illustrations that might be adduced, and arguments advanced in favour of its probability. For instance, it is a well-known fact that the first attack of colonial fever to which Europeans are liable on their arrival in the tropics is, as a rule, much more severe than succeeding ones; and common experience only confirms this well-attested fact in referring to the attack under the special name of the "seasoning fever." The name seems to imply a vaguely understood, and hitherto, so far as I am aware, unexpressed, belief in the influence of some such law as it has been attempted above to formulate. The effects of the "seasoning" seem to pass off in time; and it is interesting to observe that an old colonist, after having been in Europe for a couple of years, and having had his constitution renewed, as he expresses it, by a stay in his native land, is apt to have, on his return to the tropics, an attack of fever which is only less severe than that he had on going there for the first time, but which in its turn adapts him for the succeeding milder attacks of the intermittent which he frequently experiences. Again, it may perhaps be the case that we ought to explain on some such hypothesis as this, the fact that in an epidemic of any continued fever, the cases are as a rule much more severe at the commencement, diminishing in intensity as the epidemic advances. So, also, a second attack of the same continued fever, occurring, as it occasionally does, in the same patient, is almost invariably much milder than the first. Again, are we to say it is altogether a coincidence that the same virus which would cause a true, hard Hunterian chancre in a person who had not previously suffered from syphilis, will cause only a soft, and what is termed non-specific, sore in one previously inoculated?

This mode of reasoning, if correct, and founded on true data, may effect, it need hardly be said, important changes in our practical dealing with specific disorders; and it may not perhaps be altogether utopian to hope by means of it to relegate the still deadly typhus to the region of the less severe continued fevers. For might it not be possible to diminish materially its virulence by the induction of some disorder bearing to it a relation somewhat similar to that which vaccinia bears to variola? What the relation may be is no doubt the difficulty; but it is possible that there may be among the lower animals, or in other circumstances among human beings, some disease, the induction of which might effect the desired end. As in all other laws in physiology, and in all departments where the phenomena are variable, so in this,—it is true only within certain limits, else we should not find a patient sinking, for example, from repeated attacks of intermittent fever: but that there may be something in the suggestion is probable, and it is much to

be wished that a sufficient number of observations on the point were collected.

In the accounts given by various of the Scotch physicians of the great epidemic of 1843 and succeeding years, we have an accurate and able history of the epidemic, and it is interesting to notice how minutely the cases seen in Bradford correspond with those of 1843. The headache, vomiting, thirst, sleeplessness, quickened pulse, yellow furred tongue, with red tip and edges,¹ and "the astonishing and unexpected transition from violent excitement to a regular pulse and cool skin," with the invariable, or almost invariable, relapse, all read like an account of one of our own cases. Indeed, the unchangeableness and marked individuality, so to speak, is very striking; for while at one time the description of the symptoms observed in 1843 reminds us of those seen in Bradford in 1870, at another the account of "the discharge² of blood from the mucous surfaces, the menstruation for the first time, and the oozing of blood from the ears, with the tendency to miscarry," seems almost like an extract from Hippocrates. And even in the matter of prognosis the same holds good, a fatal event ensuing when there was oozing of blood from the ears in 1843, as when the course of the malady was observed by the physician of Cos.

The Scotch epidemic of 1847-8 presented much the same features as that of 1843, with the exception that there were more typhus cases in proportion. Thus, in 1847, we find from the Glasgow Infirmary Report that there were admitted into that Infirmary 2399 cases of typhus, and 2333 "epidemic fever" cases, with a mortality of 510 and 149, or 21·25 and 6·5 per cent. respectively. In London there was a still larger proportion of typhus to relapsing fever cases, the number of the latter not being altogether over 100.

After 1847-8, relapsing fever seems to have disappeared from this country, with the exception of a very few cases, most of which occurred in London in 1851, until the attention of physicians was again directed to it in 1869-70. Thus, writing in 1862, Dr Murchison says: "Both in London and Glasgow I have reason to believe that not a single case of relapsing fever has been observed since 1855."³ In 1869-70, the fever made its appearance in many of the large towns in the kingdom, visiting Edinburgh and Glasgow and Bradford, but being most severely felt in London and Liverpool. In the last-named city it prevailed extensively, and Dr de Zouche, who seems first to have called attention to its existence, noticed some cases as early as September 1869. Before the end of 1870 no fewer than 4000 cases had occurred in Liverpool alone.⁴ As the dates of the charts appended will show, the Bradford epidemic (which was never extensive, and prevailed only among the poor) was then at its height. The leading symptoms observed in Liverpool were sudden invasion, with particularly

¹ Reid, 1843.

² Reid.

³ Continued Fevers, p. 298.

⁴ Lancet, Nov. 12, 1870.

marked rigor, and very severe prostration, which would have been alarming if there had been any doubt as to the diagnosis, and a relapse occurring from the twelfth to the eighteenth day. The cases of a second or third relapse were not numerous. Pregnant women always aborted, and the foetus was invariably dead. The sequelæ were very distressing.

Etiology.—So far as I saw, the male sex was much more liable to relapsing fever than the female. Dr Murchison gives the proportion as 116 to 104, but inclines to the opinion that sex has very little influence as a predisposing cause. In age, the great majority of the Bradford cases occurred in males from twenty to thirty-five years of age. Dr Murchison's mean age was 24.41. I heard of one case in a very young child, and I saw some in children from twelve to sixteen years of age. I also attended an old woman, whom I believe to have died of the disease at about the age of seventy. As to occupation, almost all the cases I saw were either among paupers or persons who were very poor. As a general rule this has been the case in all the epidemics of this disease; and the fact is so notorious as to have given a name to the fever. At the same time, it must be said that careful inquiry in Bradford often failed to elicit the fact that the sick had had to undergo any special hardships, or to endure any special privations in food. Trade was moderately good in the town, and the consequent demand for labour pretty active. In Liverpool, at the time of the epidemic, "there was full employment for every able-bodied and industrious man."¹ The patients themselves did not show any signs of having been deprived of food. Of 522 patients of whom inquiry was made, only ninety-one had been in the workhouse before, which shows at least that they did not belong to the habitual pauper class. Besides this, at the time of the outbreak, and during its height, there were, 1st, considerably fewer inmates resident in the workhouse; 2d, considerably fewer families in receipt of out-door relief; and, 3d, considerably fewer patients suffering from zymotic diseases, than in the corresponding period of the year preceding. From all these circumstances it appears that the name of famine fever is not in all circumstances a quite appropriate one. From the results of a careful inquiry instituted by the *Lancet*, indeed, it appears that causes other than privation are much more potent than is commonly supposed in inducing and spreading this fever. Mere overcrowding, as such, is not the main cause—though of course a most objectionable condition—of the propagation of an epidemic; for a comparison between Liverpool and East London shows the latter to be much more thickly peopled than the former (in the proportion of 145 to 121 persons per acre), and yet the deaths from zymotic diseases are only 1040 per annum in London East, as against 1385 in Liverpool. Comparing the gross populations, the results are still more unfavourable to Liverpool, whose population

¹ *Lancet*, Nov. 12, 1870.

is only 269,000, as against 288,000 in East London. Going into more detail, the inquiry elicits that the extensive spread of the fever in Liverpool was due to at least three great causes—viz., the sites of the houses, the condition of the drains, and the extensive prevalence of cellar dwellings in darkened and filthy courts. As to the first of these, it is most unfortunate that the evil habit of emptying decaying vegetable matter with the contents of ash-pits, etc., should have converted the natural New Red Sandstone on which Liverpool is built, and which, being easily and thoroughly drainable, is both clean and healthy, into a subsoil quite unfit for the foundation of healthy human habitations. It stands to reason that midden refuse, fish-bones, lobster-shells, potato-parings, cabbage-leaves, and old door-mats, must be a very unhealthy site for house-building, and it is indeed very difficult to devise a remedy for such a condition of things. In respect of drainage, great improvements have been effected since 1847, when it appears there were only some 90 miles of drainage to some 220 miles of streets, and no drainage at all under the 90 miles of passages then existing in the town. Now, this is all remedied, the great remaining difficulty being in the ventilation of the sewers, which regurgitate their foul gases, it is said, into the houses, while in London the regurgitation takes place into the centre of the streets. The question of the ventilation of sewers, incidentally raised here, is one fraught with very great difficulty. In regard to the third great cause of the Liverpool epidemic, cellar-dwellings, the condition of things is this: Six or eight houses are built in a small court, in the centre of which stands a privy or open cesspool, occupying one-tenth of the area of the court. The only means of access to such a court is by an opening frequently not more than a yard wide. Naturally, the place not being lighted after dark, it was and is the custom of passers-by to use it as a public privy; and so it happens that the place is constantly filthy, and quite unfit for human habitation. In addition to this, the lowest of the three rooms, which, placed one over another, form a house, is sunk some six feet below the ground; and the consequences, when the surface washings percolated through the subsoil, and filtered into pools on the floors, may be imagined. The only wonder is that the human organism should be able to thrive even as it does, when exposed to such conditions.

Exciting Causes.—Contagion.—The question of the contagiousness of relapsing fever has been very fully gone into by Dr Murchison, who proves his position that it is an exceedingly contagious disorder in the same way that he does that of typhus. Several facts in corroboration of his opinion have come under my notice. Thus, several cases were admitted to hospital from the same house or street, under circumstances which left little or no doubt that several of them had been infected by others. In one case, again, early in the epidemic, before I was accustomed to recognise and diagnose the fever, I ordered a patient suffering

from it into the general ward, thinking he had rheumatism. It was not long before I had to order the removal of him and another patient to the fever hospital. Both had relapsing fever, and the other patient had been some time in the general ward. Our medical officers, nurses, and attendants on the sick, were very fortunate none of them taking the disease.

Symptoms.—In Bradford the invasion was as a rule gradual. Cases no doubt did occur in which the patients were attacked suddenly. Thus one man, not himself a smoker, was attacked while sitting among a number of smoking companions, the attack commencing with rigors, vomiting, and purging. But this was comparatively rare. Far more commonly the patient had been ailing for some time before giving in to the attack, and would present himself with a white, moist tongue, and complaining of a general feeling of malaise, and of rheumatic pains in the limbs and joints, with headache. The invasion was not so gradual as to make it difficult to discover when the disease commenced, but it was not by any means so sudden as, for instance, Dr Murchison describes it—by no means so sudden as the onset of typhus or pneumonia. Thus the initial rigors, slight in themselves, would succeed a period of perhaps some weeks' duration, during which the patient had been unable to work, and had been compelled to spend all his savings. After being put to bed, he would complain of headache, generally referred to the back part and vertex, and not, as a rule, accompanied by delirium. In most cases the conjunctivæ were yellow, and in some also injected. The face was flushed, but with this exception the skin was generally of a yellowish hue. As a rule, there was no rash, but several cases occurred in which a rash of rose-coloured spots, which could not be petechial,¹ as it generally disappeared in a day or two, made its appearance on the thorax, abdomen, and limbs. In one case (that of T. B., who afterwards took typhus), a rash was twice seen during the attack, disappearing both times in a day or two. Generally speaking, the tongue remained moist, and covered with a yellowish white fur. It was frequently red at the tip; and, while sometimes brown and dry in the centre, in only one case did I see the whole organ brown and dry, as in typhus. There was almost invariably frequent and persistent vomiting of greenish-yellow matter. This symptom was very difficult of treatment, frequently remaining constant during the whole of the primary fever, disappearing during the intermission, and reappearing during the relapse. Patients usually complained of thirst. The bowels were rather constipated as a rule, though in some cases there was diarrhœa, and in others the bowels

¹ On this point it may be mentioned that Dr Murchison has collected some cases in which a rash occurred. In all of these but four the rash was petechial, and not a true rash. In the other four cases other explanations are given, one of which is, that the appearance was due in some of the four to urticaria. I am certain that what I saw was not urticaria.

acted every other day or so. Generally the pulse was above 100, often rising to 136, and rarely to 160; though even this very high limit did not seem necessarily to indicate danger. The pulse usually ranged higher in the relapse than during the initial fever, and attained its highest point just before, at the crisis, it suddenly fell to perhaps half its rate. There was not, as in typhus, a marked diminution in the strength of the first sound of the heart. The temperature presented many curious fluctuations, to which I shall again refer. Meantime, it may be said that it varied from 102° F. to 105° during the initial fever, averaging 104°. The respiration was generally hurried, and ranged from twenty-eight to forty a minute. There was almost invariably a distressing cough, and on auscultation rhonchus and harsh breathing were made out. There was seldom expectoration, and in no case were true pneumonic sputa observed.

These symptoms continued for several days (five to nine), the patient complaining most of the nausea and sickness, and of the pains in the limbs and joints, which were very severe. The yellow appearance of skin being maintained, it was found in nearly all cases that the area of hepatic dulness was increased, while that of the splenic was always so. In some cases the splenic dulness was actually continuous with that of the heart. For the five to nine days of the primary fever, the symptoms pursued a progressive or climacteric course. The temperature and pulse would, at the end of that time, be at about their highest point, when suddenly a crisis occurred. There was a copious perspiration, or diarrhoea, or epistaxis, or two or more of these phenomena, often ushered in by a rigor (during one of which, by the way, the temperature was as high as 105° F., confirming the well-known fact that subjective sensations are no safe index to objective reality), and immediately the patient felt quite well. The temperature having fallen through a space of from 7° to 10° F., and the pulse having come from its height of 160 to 80, or from 136 to 70 or so, the patient, who a few hours before had been seen tossing helplessly in the height of the fever, or even with paralysis of the fauces and sphincters, would suddenly declare himself to be quite well, and be with difficulty prevented from getting up. The rheumatic pains, vomiting, headache, and all the febrile symptoms having disappeared, he would profess great incredulity on being told there was another period of illness in store for him; and though one might succeed in inducing him to remain in bed for a few days, he could not be prevented from getting up before the relapse. The period of exemption from febrile symptoms—the intermission—was of variable duration, usually lasting longer in those who defervesced early after the primary fever, and a shorter time when the primary fever lasted longer. Thus the intermission extended to between nine and ten days in a case where the primary fever endured only six days, and only six days when the primary fever lasted between eight and

nine. But this rule was not invariable. After an interval, however, longer or shorter as the case might be, but varying from six to between nine and ten days, the patient had another rigor, which proved the beginning of another attack very similar to the first. This relapse occurred, so far as my observation extended, in every case without exception.

The symptoms, similar to those of the primary fever, were often more severe, and one might now sometimes see the tongue completely brown and dry as in typhus, and observe the pulse ranging at what in other circumstances would be considered the perilously high rate of 160. At the end of three or four days this set of symptoms usually came to a termination nearly, if not quite, as abrupt as that of the primary fever, the thermometer indicating a fall of 6° or 7° F., rarely of 10°, and the pulse a corresponding diminution. The second crisis was generally the first step in the establishment of complete convalescence, though sometimes a second relapse occurred. In this case the symptoms were not so severe, nor the defervescence so sudden as before. It may be mentioned here, though it will be again noticed more at length, that during convalescence, as well during the intermission as after the relapse, the temperature and pulse both invariably sank considerably below the normal, gradually rising as convalescence became more completely established, until the normal was again reached. In none of the cases which I saw did a third, fourth, or fifth relapse take place, nor did I hear of any such cases in Bradford. The only fatal case I saw was that of an old woman of seventy. She died, like one of Hippocrates's fatal cases, on the sixth day. As she did not live long enough to have the relapse, there was just that amount of doubt as to the diagnosis which must attach to every such case; but we judged (I did not attend the case alone) from the symptoms, that the malady was of this nature.

I shall now proceed to remark more at length on some of the symptoms which seem to demand more careful attention; and first: The physiognomy was more flushed and red than is seen in typhus, but the conjunctivæ were less injected, and there was an absence of the stupid expression and the deafness so characteristic of that disease. The yellow cast of countenance was almost peculiar to, and characteristic of, relapsing fever.

The Rash.—So far as I can make out, the Bradford epidemic has differed from all other epidemics of relapsing fever seen in this country, by the appearance of a rash which has been more than once referred to. In the case of J. O., for instance (who, it may be here remarked, had a second relapse), it is mentioned in the notes taken at the time that on the fifth day of the primary fever, a rash appeared of small-sized (smaller, that is, than those of typhoid fever) rose-coloured spots on the thorax, abdomen, and arms. That this appearance was a real rash and not petechial, is apparent from the fact that on the next day it had disappeared. In another

case, again, it is noted that twice in the course of the fever a rash appeared which was not distinguishable from that of typhoid fever. It was rose-coloured, and the spots measured from 1 to 2 lines in diameter. The second appearance of the rash was just before the deservescence at the end of the relapse. This rash also remained only a day or two. In reference to the appearance of the rash and its similarity to that of typhoid fever,—a similarity which is all the more remarkable that Dr Murchison says it has never before been observed in relapsing fever,—it is interesting to notice that when the patient took typhus immediately after having had relapsing fever, the rash that appeared on the fifth day of the typhus was much more like the rash of typhoid fever than of typhus, being rose-coloured; but the spots were much smaller than those seen in typhoid. I refer to this here, partly because of the interest which, it seems to me, attaches to the observation, and partly because it seems to have some bearing on the views already expressed in reference to the sequence of one specific fever on another in the same patient at a short interval of time. For, besides the fact that even the characteristic typhus rash seemed to be modified by the previous attack of relapsing fever, and assimilated to the rash that was then noticed, it is a curious fact that the delirium which is so common and important a feature in typhus was much less severe than usual, while it was very acute in the previous relapsing attack. These facts appear to go to show that the course of the typhus had been modified and ameliorated by the previous course of relapsing fever, in accordance with the terms of the general law which it has already been attempted to lay down. In this connexion, as well as on its own account, the case of M. J. D. seems deserving of special study. It ran on for thirty-six days; and I have called it, subject to correction, a case of relapsing-enteric fever. The only other view one can take of it is that it was another case of typhus following relapsing fever. In favour of the former view are the facts that the patient had the muscular and arthritic pains, the nausea, vomiting, and general abdominal tenderness, increased splenic dulness, uterine activity, as shown by the appearance of menstruation, characteristic of relapsing fever; while the thermometric readings, though not so characteristic, seem to approximate more nearly to that than any other fever. Along with these symptoms coexisted a rose-coloured rash appearing on the fourteenth day, and more or less purging throughout the attack—symptoms which, taken with the red-tipped tongue, agree with those of enteric fever. In favour of the view that we have a case of typhus following relapsing fever, we must consider these facts. What may be called the relapsing fever ran on for the usual course of seventeen days (on this view we should have here another case of relapsing fever in which rash occurred). On the twentieth day the typhus began, and continued till the thirty-second day from the initial rigor. But there was no rash present during what may be considered the typhus, and for this reason I have preferred to look on it as a case

Nov. 1869.

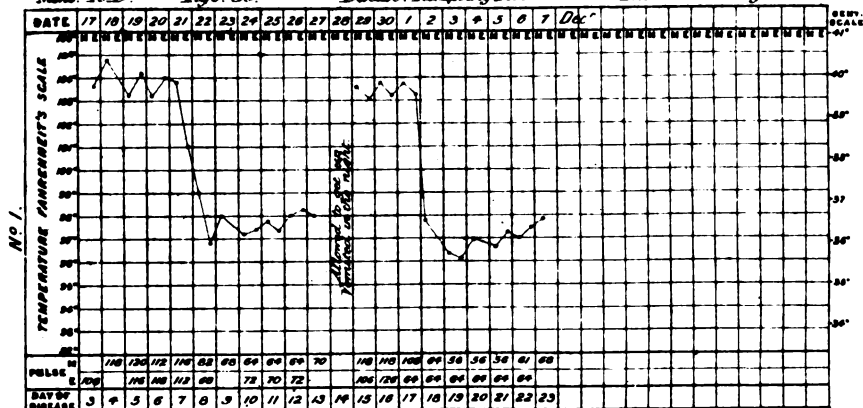
CLINICAL CHARTS OF TEMPERATURE. Dec.

Name: R. D.

Age: 30.

Disease: Relapsing Fever.

Result: Recovery.



History.—Was ill a week before admission; had saved some money, and did not suffer any special hardships. Symptoms.—Patient very yellow; epigastric tenderness; tenesmus; pains in limbs; tongue white and moist throughout; vomiting persistent. Relapse ushered in by rigors and vomiting.

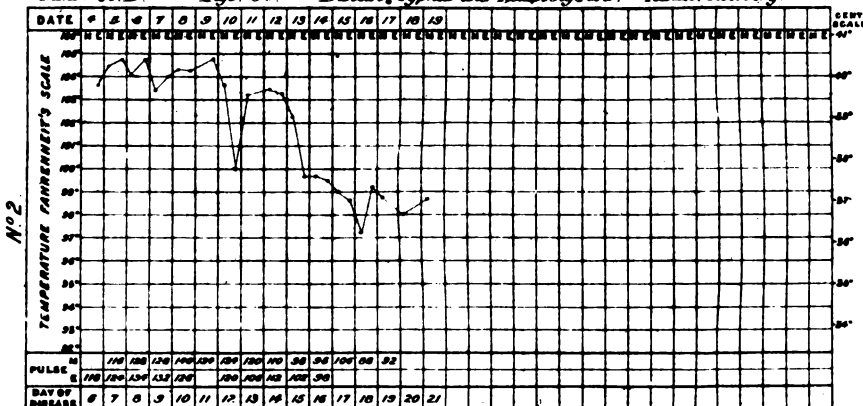
Jan. 1870

Name: R. D.

Age: 30.

Disease: Typhus after Relapsing Fever.

Result: Recovery.



On Dec. 30, 1869, had rigors and headache after getting his feet wet. Symptoms.—White moist tongue; vomiting; epigastric tenderness; arthritic and muscular pains; skin yellow. Rash on Jan. 6, the 8th day Jan. 13. Violent delirium.

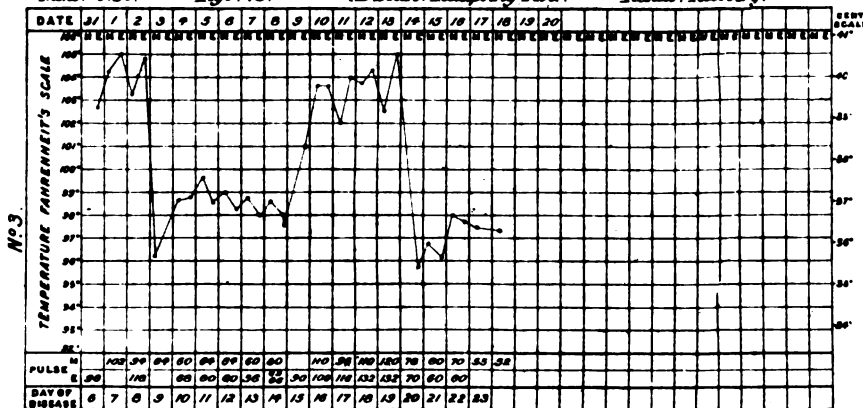
Died: 1869. Jan. 1870.

Name: C. H.

Age: 18.

Disease: Relapsing Fever.

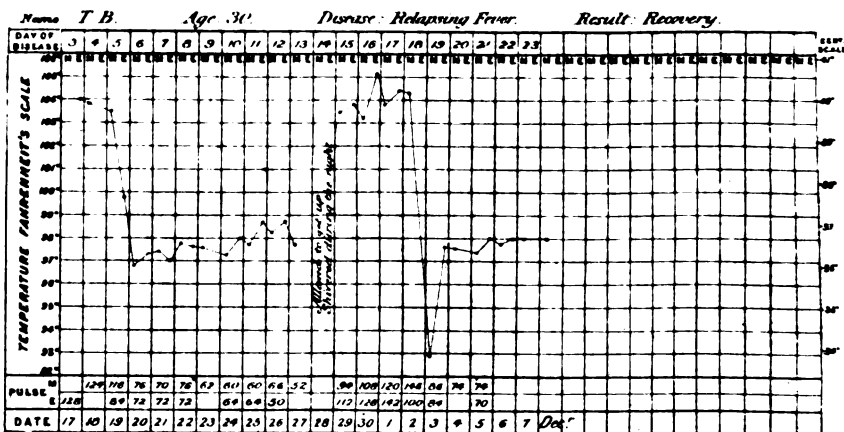
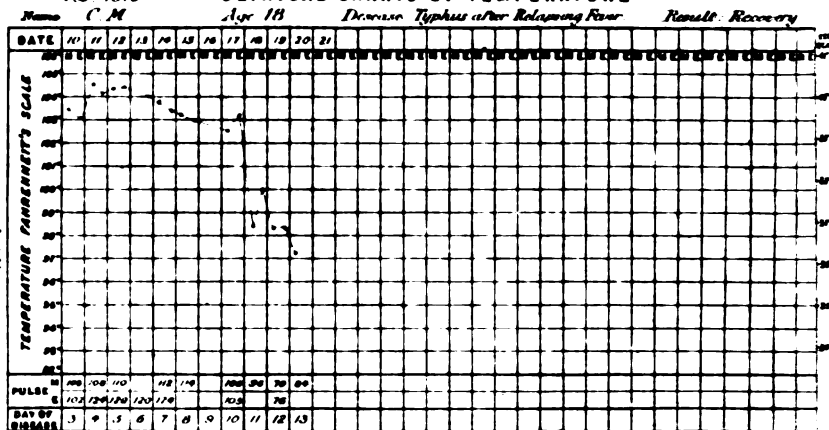
Result: Recovery.



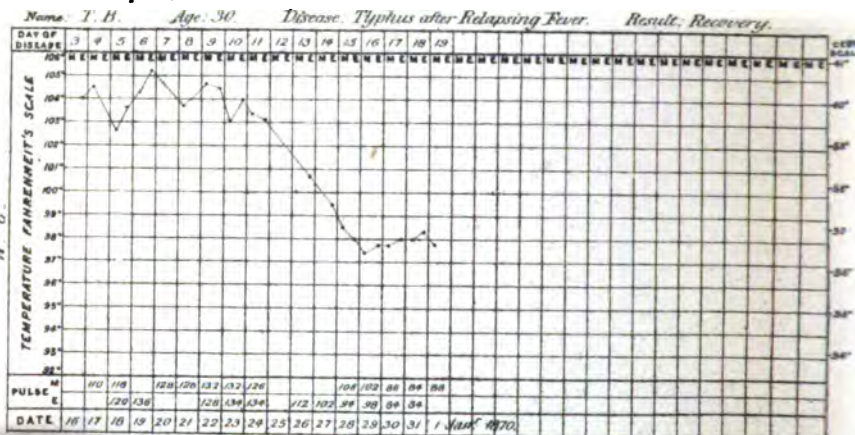
Rigors on Dec. 28.—Jan. 1. Headache, shifting from place to place; tongue dry, but with white loaded centre and red edges; severe arthritic pains; epigastric tenderness; sclerotics yellow conjunctivae injected.—Jan. 3. Profuse sweating.—Jan. 10. Tongue brown, dry, other symptoms as initial fever.—Jan. 14. Took a hearty breakfast, and expressed himself as feeling quite well.

Feb 1870

CLINICAL CHARTS OF TEMPERATURE



Nov. 1869 History.—For eleven weeks before admission, was out of work and often without food. Symptoms.—Yellow appearance; epigastric tenderness; arthritic pains. N.B.—Twice during the illness a rose-coloured rash appeared; spots 1 to 2 lines in diameter. Bowels constipated.



18. Whole skin except face covered by a rose-coloured rash, fading, but not disappearing, on spots not so large as those of typhoid fever. Somewhat delirious till the 24th.

No 9.

Jan^y 1870.

CLINICAL CHARTS OF TEMPERATURE

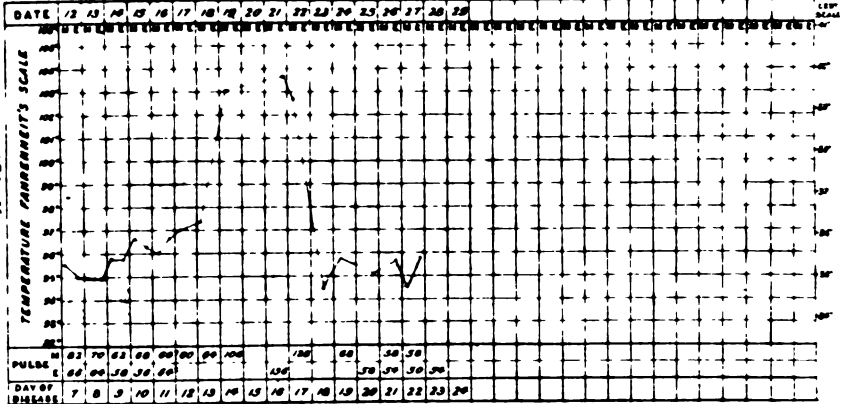
Name *M. C.*

Age *24*

Disease: *Relapsing Fever*

Result: *Recovery*

N^o 10



Note the persistently low temperature in this case. A second relapse occurred on Feb. 1, lasting three days; during it the temperature rose to 104° F.

Jan^y 1870.

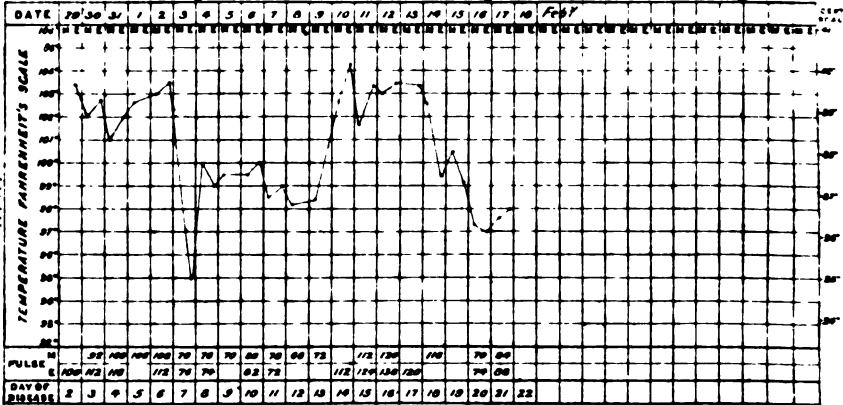
Name *J. C.*

Age *45*

Disease: *Relapsing Fever*

Result: *Recovery*

N^o 11



Invasion sudden: no sweating at crisis.
N.B.—The rise in temperature during the intermission.

Jan^y 1870.

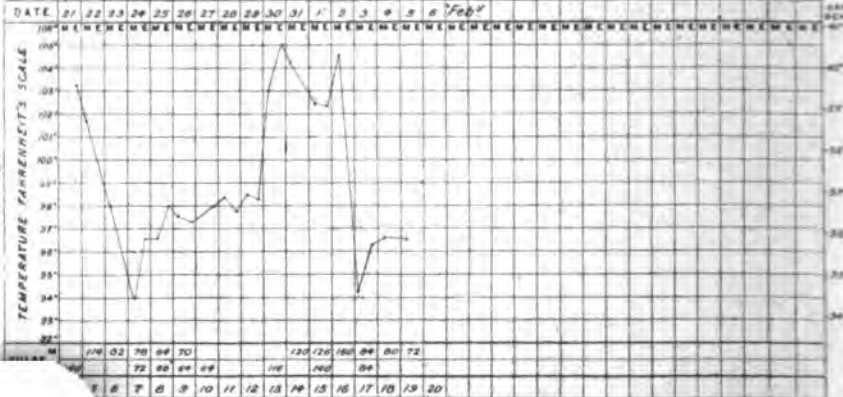
Name *W. W.*

Age *27*

Disease: *Relapsing Fever*

Result: *Recovery*

N^o 12



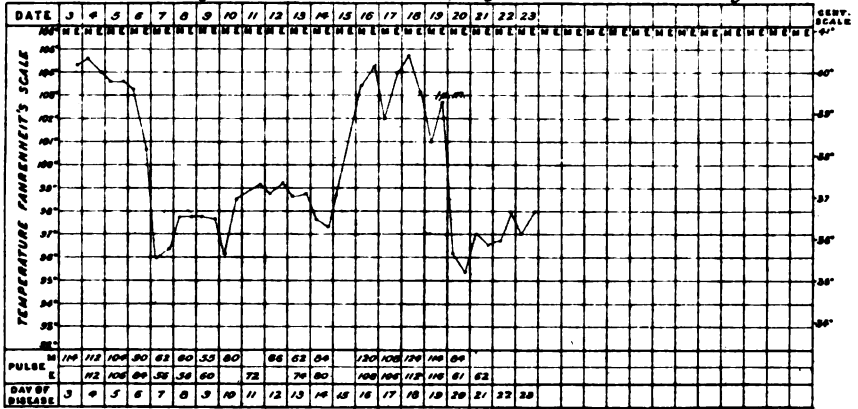
that had intermittent fever in India, and could not be persuaded he had not a return of the

March, 1870.

CLINICAL CHARTS OF TEMPERATURE

Name: *John C.* Age: *25.* Disease: *Relapsing Fever.* Result: *Recovery.*

No 13

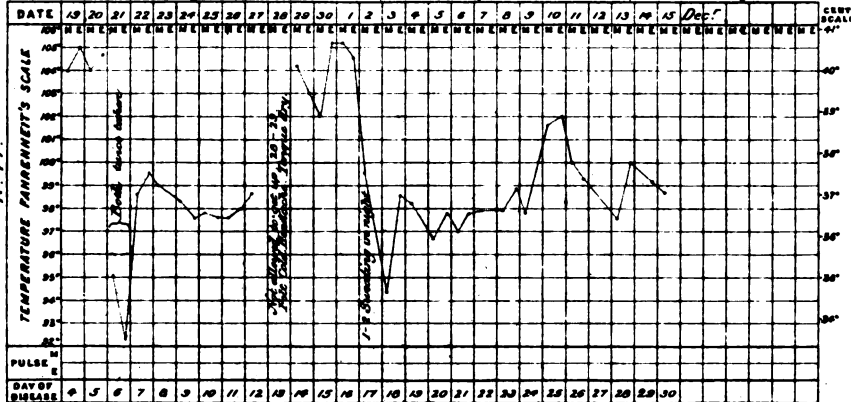


Invasion sudden.

Nov. 1869

Name: *J. O. H.* Age: *20.* Disease: *Relapsing Fever.* Result: *Recovery.*

No 14

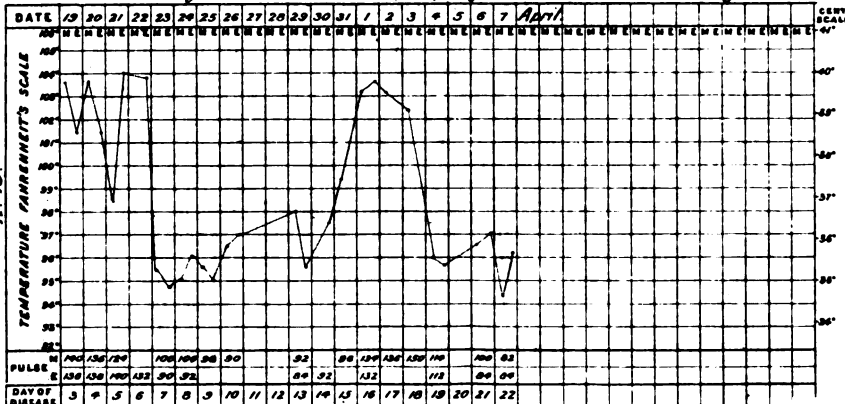


Nov. 20. Rose-coloured rash of small-sized spots on abdomen, thorax, and arms.—Nov. 21. Rash disappeared; sudden and great defervescence, with sweating.—21-22. A very delirious night

March, 1870.

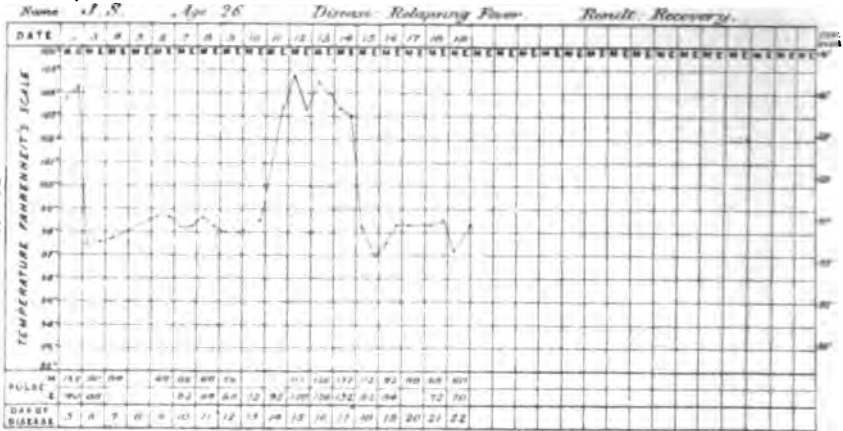
Name: *S. S.* Age: *30.* Disease: *Relapsing Fever.* Result: *Recovery.*

No 15



April 1870

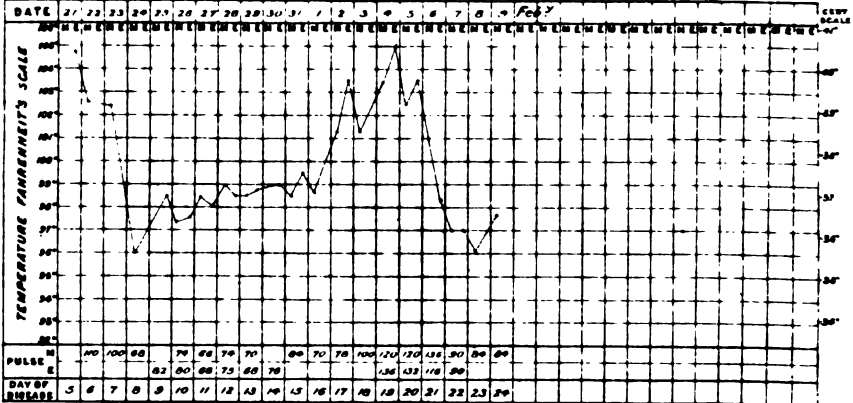
CLINICAL CHARTS OF TEMPERATURE



No 16

Jan^y 1870.

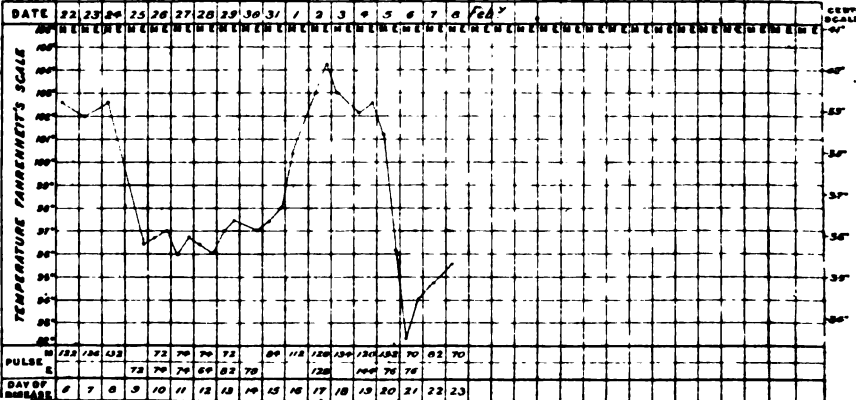
Name: B. B. Age: 34 Disease: Relapsing Fever Result: Recovery



No 17

Jan^y 1870

Name: H. J. Age: 35 Disease: Relapsing Fever Result: Recovery



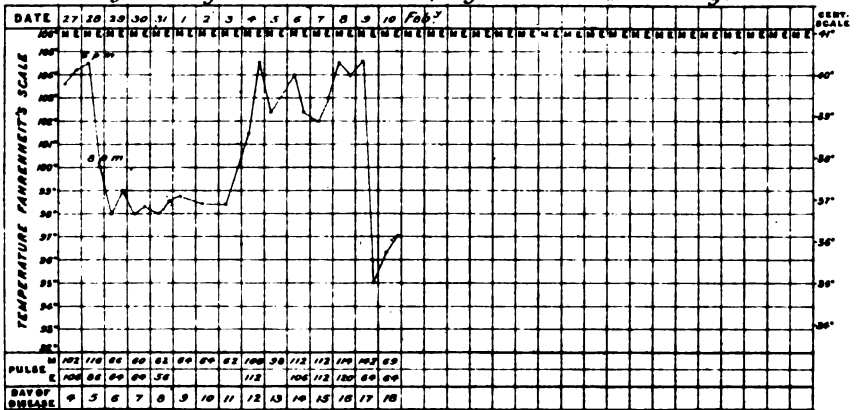
No 18

Jan^y 1870

CLINICAL CHARTS OF TEMPERATURE

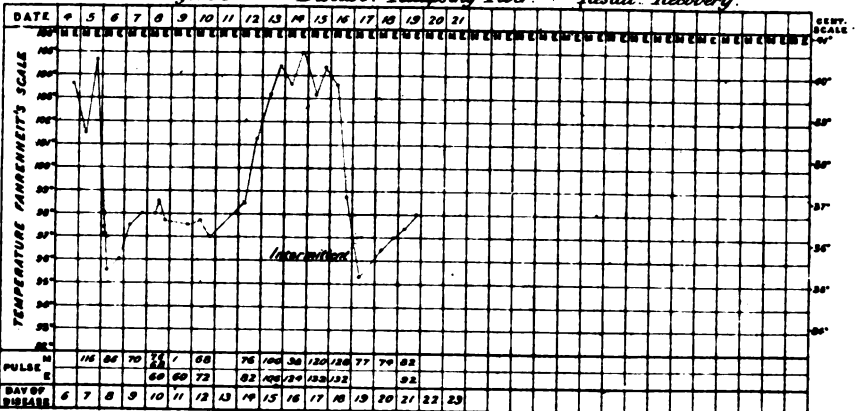
Name: J. Q. Age: 25. Disease: Relapsing Fever. Result: Recovery.

No 19.

Jan^y 1870.

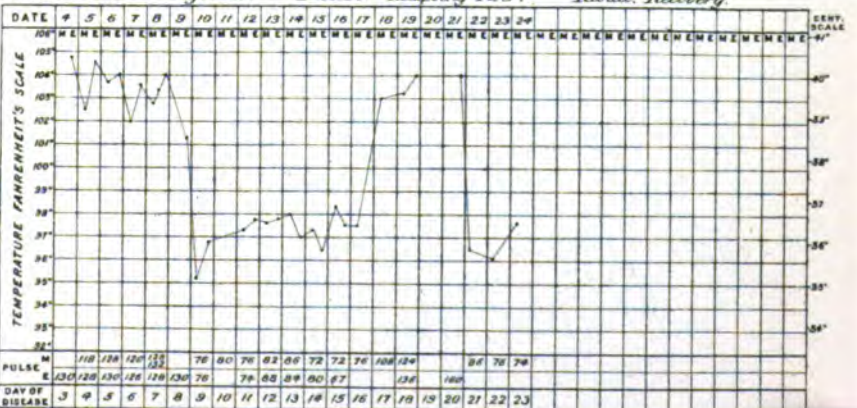
Name: J. B. Age: 33. Disease: Relapsing Fever. Result: Recovery.

No 20.

Jan^y 1870.

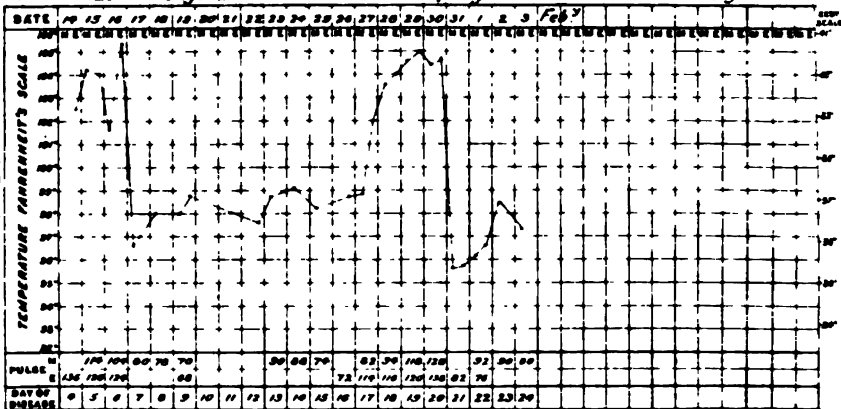
Name: G. A. Age: 32. Disease: Relapsing Fever. Result: Recovery.

No 21.



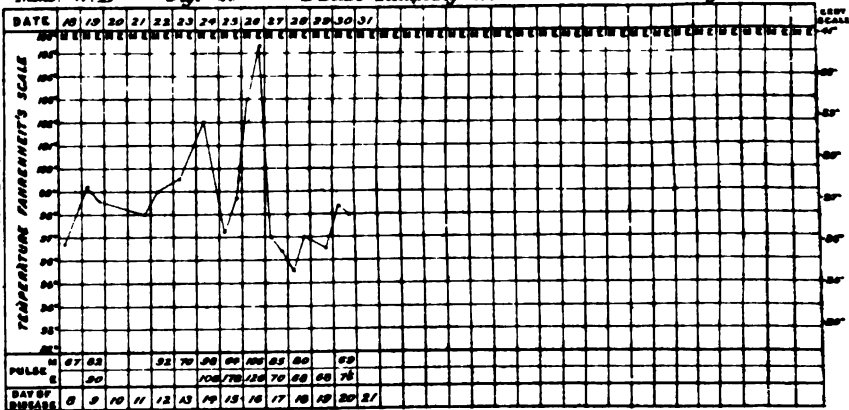
Edinburgh Medical Socy.

№ 22



Result Recovery

№ 2.3.



of enteric fever complicated with relapses. The interesting point to be observed, however, is, that whichever view we take of this, affords an additional argument for the existence of the law referred to—on the one supposition that the enteric fever was modified by the epidemic of relapsing fever into taking on a relapsing character—on the other, that the succeeding typhus was modified by the preceding relapsing in a very peculiar manner, taking in temperature a relapsing character, and being rendered much milder than it probably would have been had the patient been attacked by typhus in the first instance.

I saw no petechiæ nor vibices. One or two cases had an abundant crop of sudamina about the period of crisis. These have been noticed by Douglas and other observers. It does not seem that much importance should be attached to them.

Moisture of Skin.—Sweating, in the cases I saw, as in all described cases of relapsing fever, was a common and characteristic symptom. It was specially profuse at the crisis, coinciding with a very marked fall both in pulse and temperature. (See charts, *passim*.) Whether the fall of temperature was causally connected with the perspiration, or whether the two phenomena were merely accidentally conjoined, is a point difficult of determination. Wunderlich¹ mentions “profuse perspiration” as one of the means of reducing temperature in the fastigium and amphibolic stages. But when the perspiration is slighter the fall in temperature is very frequently not observed. Thus, in peritonitis, puerperal metritis, phthisis, etc., sweating, though a common symptom, is not accompanied by a fall in temperature. The change from the gaseous watery vapour to condensed water, coinciding as it does with a development of latent heat, would almost lead us to expect an increase of temperature, supposing the conditions as to radiation to be unchanged. Or it may be that we should look on the development of perspiration as so much conversion of energy, which would explain the descent of the mercury; but this whole department, and the scientific relations subsisting between physical and physiological phenomena, awaits investigation. The reaction of the perspiration, as of the urine, was acid—this condition of things coinciding with rheumatic pains in the limbs and joints.

Temperature.—The records of temperature in a case of relapsing fever seem to me peculiarly interesting and deserving of study, showing a set of phenomena which it is impossible to confound with any other with which I am acquainted. They may be studied as characteristic of the relapsing type of disease, and may be defined roughly as two or more sets of high readings in temperature, alternating with periods of complete apyrexia. A typical case (see C. M.) may be described as follows:—For a period of from five to nine days the temperature ranges from 103° to 105° or 106° F., varying a degree or two between night and morning, just as is ob-

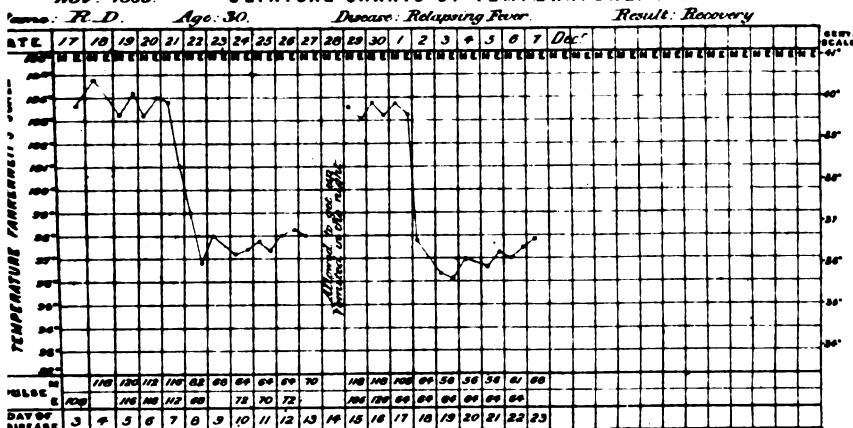
¹ Medical Thermometry, Woodman's translation, p. 264.

served in continued fevers. I have, unfortunately, few observations (see, however, chart of J. C.'s case) on the commencement of fervescence; but a peculiarity of it, according to Wunderlich,¹ is that the rise is very rapid, so agreeing with the early stage of typhus, but differing very decidedly from the same stage of typhoid. About the fourth day (see charts, J. O'H. and T. G.) the elevation is often at its height, so that the record presents a peaked appearance, after which it slightly declines to the crisis, at which point a sudden and most extensive fall takes place. Thus, in C. M.'s case the fall was from 104·7° to 96·2°, or 8·5° F. In J. O'H.'s and T. G.'s cases the fall was respectively 9·6° and 6° F. Sometimes it is as great as 12° F. (Wunderlich). So far as my observations went, this fall invariably brought the temperature below the normal, a point which, though mentioned by writers (see Wunderlich) as an occasional occurrence, does not seem to have been stated so emphatically as its importance would appear to demand. Having fallen so considerably, the temperature as a rule gradually rose to normal (see cases of T. B., etc.), at or about which point it stood before the relapse suddenly raised it again. But many exceptions to this rule occurred, and frequently (see the case of C. M.) it rose somewhat rapidly till it slightly exceeded normal, when it might descend a little. In other cases (case of T. G.) it considerably exceeded the normal, while in yet others (S. S. and M. C.), it remained quite 3° F. below the normal until the relapse. At this period, which was usually about the fourteenth day, it rose somewhat suddenly, the variation between night and morning of the relapse being as great sometimes as 8° or 10° F., and this elevation was maintained for a period of from three to five or six days. There was a variation, as in the primary fever, of a degree or two between night and morning, while the diagrammatic representation of the temperature in the relapse, as in the primary fever, frequently showed a fastigium. (Case of T. B.) The highest elevation was generally shown about the second or third day in the relapse. In some cases there occurred a sudden and rather extensive fall of temperature during the relapse. This, lasting for one or two days, gave to the disease the appearance of a tertian or quartan character, and may perhaps account for, if it does not justify, the seeming confusion of Hippocrates in describing the disease as he saw it. For typical instances of this intermission during the relapse, see cases of T. G. and W. B. It may be observed in a less exaggerated form in the charts of other cases. As a rule, the temperature during the relapse attained quite as high, and sometimes even a higher elevation than in the primary fever, and the defervescence was frequently quite as sudden. Thus, in C. M.'s case it fell 9·3° F. The defervescence after the relapse usually rendered the temperature subnormal, sometimes by several degrees. The rule then was for the temperature to rise till the line of health was reached, when convalescence was established. In the

¹ Medical Thermometry, p. 333. Cf. pp. 300, 301.

Nov. 1869.

CLINICAL CHARTS OF TEMPERATURE. Dec.



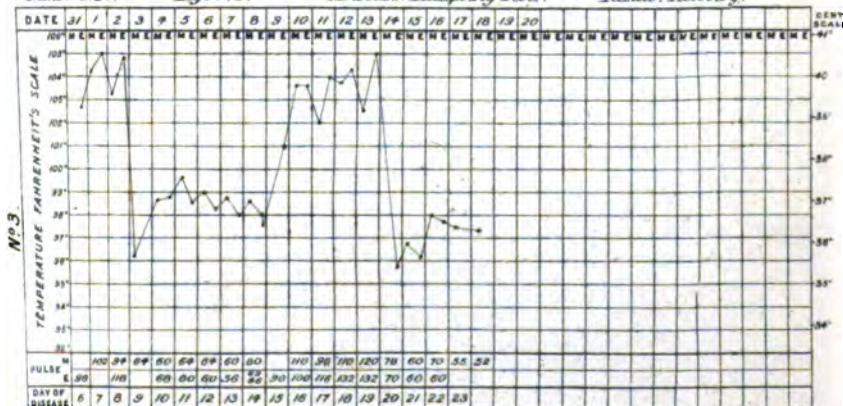
History.—Was ill a week before admission; had saved some money, and did not suffer any special hardships. *Symptoms.*—Patient very yellow; epigastric tenderness; tenesmus; pains in limbs; tongue white and moist throughout; vomiting persistent. Relapse ushered in by rigors and vomiting.

Jan. 1870

Name: *R. D.* Age: *30.* Disease: *Typhus after Relapsing Fever.* Result: *Recovery.*

On Dec. 30, 1869, had rigors and headache after getting his feet wet. *Symptoms.*—White moist tongue; vomiting; epigastric tenderness; arthritic and muscular pains; skin yellow. Rash on Jan. 6, the 8th day Jan. 13. Violent delirium.

Died: 1869, Jan. 1870.

Name: *C. M.* Age: *18.* Disease: *Relapsing Fever.* Result: *Recovery.*

Rigors on Dec. 26.—Jan. 1. Headache, shifting from place to place; tongue dry, but with white loaded centre and red edges; severe arthritic pains; epigastric tenderness; sclerotics yellow; conjunctiva injected.—Jan. 3. Profuse sweating.—Jan. 10. Tongue brown, dry, other symptoms as in initial fever.—Jan. 14. Took a hearty breakfast, and expressed himself as feeling quite well.

its existence coinciding with the pains in the limbs and joints often rendered the diagnosis obscure, particularly in those cases in which the initial rigors were not well marked. In one or two cases the whole tongue was brown, baked, and dry during the relapse, but much more commonly only the central portion presented this appearance. The vomiting I have already remarked upon. I saw no black vomit.

Enlargement of the liver and spleen occurred frequently. In some cases the latter organ was immensely enlarged, the dulness indicating the existence of a viscus like another liver. In such cases the patient complained of weight and tenderness at the part, but there were no symptoms to indicate inflammatory action. The tenderness over the liver was often marked, occurring with jaundice, which was present in nearly every case. As the latter was so common, occurring as well in slight as in severe cases, I cannot agree with those observers who look on it as a formidable symptom. It is nearly certain that it has no connexion with head symptoms, and depends, as Dr Murchison suggests, on functional interference by the fever poison with the proper metamorphosis of the biliary products, which are retained in the blood in place of being eliminated by the liver.

I am inclined to connect the muscular and arthritic pains which are so troublesome in relapsing fever with the acid reaction of the perspiration and urine, and to consider them, with the furred white tongue, as indications of a general rheumatic condition. They were relieved by alkalies. In London patients were treated by acids, but as nitre was recommended at the same time, the good effects of the treatment might be due to that.

In the only case of muscular paralysis with coma due to the fever which I saw, it is an interesting fact that, although paralysis affected the limbs, sphincters, and even fauces, the patient recovered. The patient had, besides, jerkings of the limbs, and when at the worst became bathed in a profuse perspiration—conditions which would have been of the gravest import in typhus. When the same patient took variola immediately afterwards the attack was exceedingly mild, and there were no head symptoms at all.

Stages and Duration.—It is surprising how much the course of relapsing fever varies. Thus, an examination of even a limited number of cases will reveal differences, not minute and unimportant, as may be seen from a comparison of different cases of all kinds of fevers, but variations of a considerable number of days. Thus, in the twenty cases whose charts are appended, the primary paroxysm varied in duration from five to nine days—in eight of the cases being over before the seventh day, which most observers fix as the average duration. So with the relapse, which fell from the eleventh to the seventeenth day from the commencement of the attack. In ten of these cases it occurred between the thirteenth and fifteenth days, which agrees very well with fourteen as the day on which,

according to most observers, the relapse may be expected. The whole duration is generally seventeen or eighteen days, which allows three or four days for the relapse. Of course this is reckoned till deferescence is established, the duration, if convalescence is included, being considerably longer. But even as regards this, variations occur, and the disease may go on for twenty or even twenty-four days. I did not see many cases in which a second relapse occurred. In those I did see it was much milder than the preceding one.

As to treatment, I think it may be taken as pretty certain that nothing will essentially modify the course of the disease. Symptoms can, of course, be relieved, but the landmarks of the malady are well marked and cannot be affected. Thus I tried several anti-periodics to prevent the relapse if possible, but failed to get decided results from any of them. The headache and arthritic pains were very severe, and though both could sometimes be relieved by alkalies, it was often necessary to have recourse to opium. Colchicum seemed to have no effect on the pains. The persistent and distressing vomiting required all the resources of art for its alleviation. Sometimes the effervescing draught, with or without hydrocyanic acid, was of service; sometimes relief was obtained by the administration of small and frequent doses of ipecacuanha; sometimes by sinapisms to the epigastrium; and in some cases all these resources disappointed, till the patient recovered in the natural course of the disorder.

I have now described, in its symptoms, course, and duration, the epidemic of relapsing fever which appeared in Bradford in 1869-70. I have shown, that while its main features were so distinct and typical as to leave no room for doubt as to the nature of the disorder with which we had to deal, it differed in some particulars from what is recognised as the ordinary course of relapsing fever. If some of these differences, as the comparatively gradual invasion, the "vertical" position of the headache, and the absence of constipation, be thought of little moment, I think it will be admitted that in the appearance of a rash in a minority of the cases, it differed in an essential point from all epidemics of the disease heretofore seen in this country. In the interesting point of the relations subsisting between this fever and typhus, and the modifying influences on the latter which a previous attack of relapsing fever seems to exercise, it has been attempted to trace the operation of a general law in pathology, hitherto unrecognised, in its generality at least, though it has been, and is, argued that some of its particular instances are well known and recognised. In accounting for the phenomena of vaccination as a special case of the action of a general law, some additional order is evolved out of the variable phenomena of pathology; and it may perhaps be hoped that this may not be without influence on medical opinion and practice.

Nature of Relapsing Fever.—It now only remains for me to say a few words as to the nature of this fever. In speaking of the

April 1870.

CLINICAL CHARTS OF TEMPERATURE

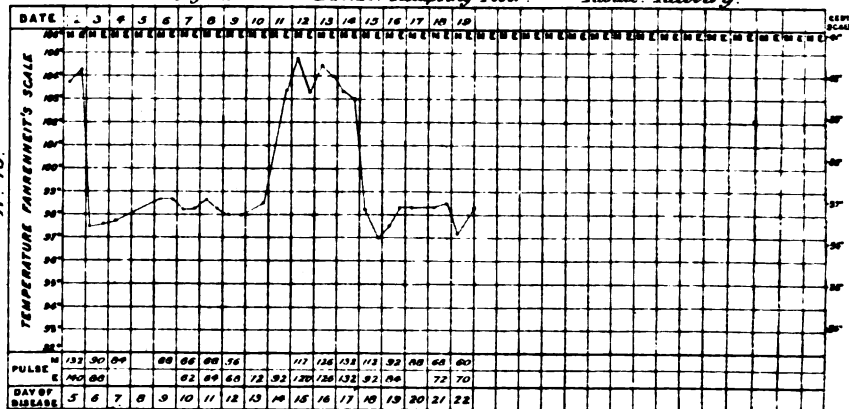
Name: J. S.

Age: 26

Disease: Relapsing Fever.

Result: Recovery.

No 16.



Jan'y 1870.

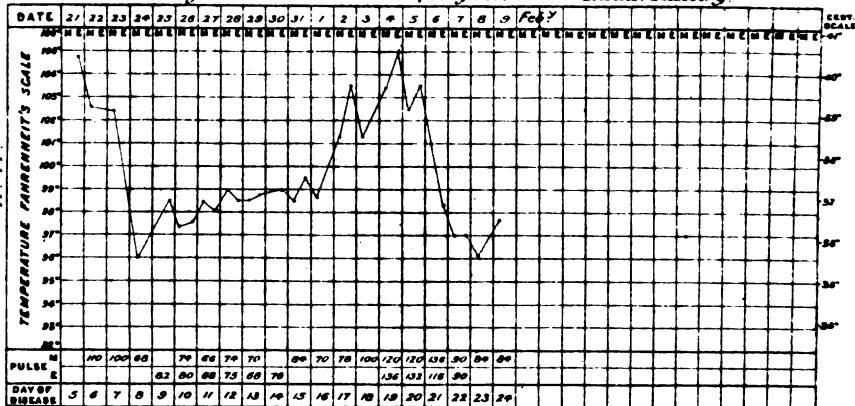
Name: B. B.

Age: 34.

Disease: Relapsing Fever.

Result: Recovery.

No 17.



Jan'y 1870.

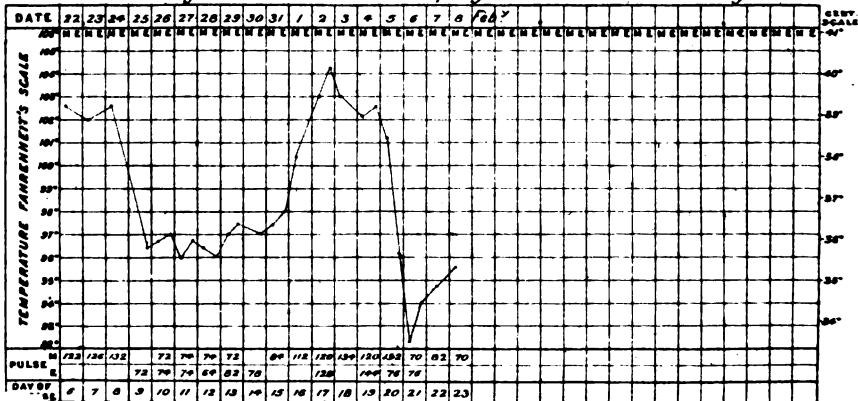
Name: H. J.

Age: 35.

Disease: Relapsing Fever.

Result: Recovery.

No 18.



and at the same time less and less well marked—become remissions in short, and continue altering in this way for the worse till the patient sinks under them altogether. Now, even here, the analogical similarity obtains, and I would refer to the cases of T. G. and J. C. as proving it in the strongest and most beautiful manner. In T. G.'s case (which, by the way, nearly proved too much for the patient, so agreeing with the dangerous nature of the remittent form), it will be seen on reference to the chart, that though the temperature fell below the normal at the end of the first crisis, as in the ordinary way, it did not long remain so. The day after, it had risen above normal, and this rise continued the following day, when it showed a record of 101.7° F. After this, it slowly declined till it attained the normal just before the relapse, which was characteristic enough, and lasted four days. The defervescence at the end of the relapse was not so sudden as usual, though nearly as extensive. But see what happens then. No sooner has the temperature come down than it darts up again in the most erratic manner, and only slowly comes down again. This case is certainly taking on the remittent character, and thus we see that not only in its normal symptoms is relapsing fever essentially an intermittent, but that it is so even in its abnormalities. Both have what may be termed their remittent form, which is also the most dangerous form in both. I do not see how the force of this reasoning can be resisted. It is as if a botanist, having been led by examination of their structure to refer two plants to the same natural order, and being confirmed in his opinion by the discovery that both had the same physiological properties, happened then to stumble in his rambles on a sport of each which followed the same erratic direction. What was opinion before now becomes absolute certainty. So we, finding a similarity in the course of relapsing and intermittent fevers, and discovering by investigation that their symptoms are essentially the same, are finally surprised to find that their very abnormalities are alike, and are therefore irresistibly compelled to refer them to the same category.

ARTICLE III.—*Remarks on the Employment of Bulbous Bougies.*

By A. G. MILLER, M.D., F.R.C.S. Ed.

(Read before the Medico-Chirurgical Society of Edinburgh, 5th November 1873.)

WHAT I have to bring before your notice on the subject of the employment of the French elastic, bulbous-pointed, urethral instruments, is mainly under two heads:—1st, When they are *not*; and, 2d, When they *are* useful. My remarks will relate entirely to my own experience, and are necessarily of a fragmentary nature.

In the first place, then, there are circumstances when these French instruments are quite useless, though they are recommended, and doubtless attempted to be used, by many surgeons. The first of

these is retention when due to a tight stricture. In such cases we all know that a certain amount of *intelligent force* is necessary. And the objection that applies to all flexible instruments, viz., that they cannot be guided, more than holds good here; for these are the most flexible of all flexible instruments, and I have known the very fine ones double on themselves when they meet with the slightest resistance to their passage.

Again, I consider these instruments useless for the treatment of stricture by simple dilatation (except in a certain manner hereafter to be described), and that for two reasons. In the first place (as you will see), the bulbous points are not properly graduated and proportioned to the size of the bougie. For instance, No. 11 may have a bulb at its extremity absolutely smaller than that of No. 7 or 8. And I consider that it is of vast importance, for the proper employment of these instruments, that the bulb should be carefully proportioned to the size of the shaft of the instrument, being at least the same size, or, better still, a shade larger, to pave the way for the easy passage of the shaft. In the second place, should the bulbous extremity pass through the stricture—it may be quite easily and with a plunge—often all your skill and force are insufficient to pass the instrument one fraction of an inch farther; it merely doubles on itself. And I have frequently proved, to my own satisfaction, that where a flexible instrument has failed, a rigid catheter, of the same size, can be made to pass quite easily. It will be readily seen from this that these instruments cannot be used to dilate strictures in the ordinary manner; that is, by being passed on into the bladder, which is the most satisfactory way, and that usually practised. What I have said already will seem like truisms to most of my hearers, but I have been induced to draw attention to the subject by the manner in which flexible instruments (and more especially those to which I have alluded) have been lauded in some quarters for the treatment of stricture of the urethra.

And this brings me to my second head, viz., the use to which these instruments may be applied.

In the treatment of a stricture which is not very tight—permitting, say, a No. 6 or 7 to pass—I have often found (as I have already mentioned) that though the bulb may pass through the stricture readily enough, the shaft will not, and yet on withdrawing the flexible and substituting a rigid instrument of the same size, the latter can be got in. This result I am inclined to attribute, not only to the greater power of overcoming difficulties which the rigid instrument imparts, but also to the distending influence which the bulb of the flexible one exerts on the stricture. And applying this idea to the treatment of such strictures, I now make it my regular practice, when I have any difficulty in passing a rigid instrument (say No. 6), to pass the bulbous extremity of one of these French instruments (say the size of No. 7), and then to pass (which can be quite easily done) the rigid instrument with which I had formerly failed.

It is a very important thing, in the successful treatment of stric-

tures, to avoid violence, or undue force applied in instrumentation; and I think the proceeding which I have mentioned is a great help in this direction, for the bulb passes through a stricture so much more easily than any cylinder of the same diameter can, and at the same time can be managed with such perfect accuracy and safety, that its use cannot but recommend itself to the careful surgeon.

We all know how, when using the "protesting bougie" in the after-treatment of strictures, we are frequently disappointed to find that the stricture has stolen a march on us, and got a little tighter for the time being. In such cases, also, I think that in the method I have described we have the means of not only overtaking the lost ground, but of making actual progress. For example, you have a patient for whom you are occasionally passing a No. 9, to see that things are keeping right. Through exposure and cold, or some error in diet, or from some other cause, the stricture has become slightly tighter, and you cannot get in No. 9 as easily as formerly, or perhaps not at all, without exerting considerable force and causing pain. You pass a bulb the same size as your instrument, or perhaps a little larger, in diameter (which you *may* effect quite easily), and then you can follow that up with your former instrument, or even a larger one, after a little careful persuasion. The good effected by this procedure I attribute to a stretching of the stricture, and perhaps to the relief of spasm in some cases by the presence of the bulb in the canal of the stricture for a few moments.

What has been said as to the facility, safety, and accuracy of manipulation, with the *bulbous extremities* of these bougies, will at once suggest their employment for the treatment of strictures by simple, gradual dilatation. By measuring and arranging the instruments according to the diameter of the bulbs, it will be possible to get an accurate series from the smallest to the largest made. And the *modus operandi* would be to pass only the bulb through the stricture, and then to withdraw the instrument and pass another bulb of a size larger. In this manner a stricture might be very safely and rapidly dilated in the ordinary manner of gradual dilatation. This method I have not yet tried; I am only prepared to suggest it. But I think its recommendation might be supported by the analogous process in the treatment of stricture of the œsophagus. The method will doubtless not be applicable to all, probably only to a small class of cases, on account of the element of doubt and difficulty introduced by the fact that the instrument is not passed on into the bladder. This might, however, be so far met and overcome by the occasional introduction of a rigid instrument right on into the bladder. The employment of the bulbs only in dilating a stricture, I should imagine, theoretically, to be more useful for strictures that are more or less spasmodic in their character, and where no false passages exist.¹

¹ These instruments, may also be useful occasionally in diagnosing the situation and extent of a stricture.

There are other circumstances under which these flexible French instruments may be useful; for instance, in cases of retention from affections of the prostate. And I think that one thing in favour of these instruments is, that the patient can be easily taught to use them himself. Being scientific men and not tradesmen, we need not be so mercenary as to grudge our patients the luxury of helping themselves to make water whenever they like. In like manner, where patients live at a distance from their surgeon, they may be permitted to supplement the use of the "protesting bougie" by occasionally passing for themselves one of these flexible bougies. In this way they may visit their surgeon at greater intervals than they would otherwise require to do, or only when they meet with some difficulty. For they can do no harm at any rate, even should they be unable to effect any good; that is to say, provided certain necessary precautions are carefully observed.

A bulbous instrument, to be perfect, should have its bulb larger in diameter than the shaft, and the catheter should be perfectly smooth at the margins of the eyelet. If these two things are carefully provided for, there is no danger in the employment of the instruments. If the catheters be roughly handled, either in cleaning or in using them, they are very apt to bend at the eyelet; in this way they become cracked and ragged at the edges, and when used they scratch and irritate the mucous membrane of the urethra, and even draw blood. I have noticed, and my patients have mentioned to me also, that these soft, apparently most harmless instruments, more frequently draw blood than the ordinary silver catheters; and I have found it due generally to what I have mentioned above. The drawback is slight, the more so that no pain seems to be associated with it, but still I think it is worth mentioning.

And now a single word upon a somewhat different subject. In many cases of stricture the sufferer does not care, and subsequently is not able, to empty his bladder completely; and this condition of matters is generally proportioned to the tightness of the stricture and the length of time it has existed. I do not mean to enter upon this subject further than to say, that I would suggest two things to be followed out in the treatment; first, that *catheters* should be used instead of bougies, in order that the bladder may be thoroughly emptied; and, secondly, that instrumentation should be *after* the patient has made water (if possible), in order that the exact state of the bladder should be fully ascertained. For such cases as these the flexible catheters are well fitted, as by means of them the bladder can be very completely emptied. And it is advisable that in such cases, when the patient passes the "protesting bougie" for himself, it should be a "*catheter*" (to use an Irishism), and should be passed *after* he has made water; and the frequency in using the instrument may then be regulated by the condition of the bladder, as well as of the stricture.

ARTICLE IV.—*Fracture of the Skull, with Chronic Abscess—Death in Three Months.* By JAMES MORE, M.D., Rothwell, Northamptonshire.

AFTER reading Dr McLeod's interesting case of recovery from extensive wound of the brain in the November number of your Journal, it struck me the following case might be worth recording, as illustrating the very grave results which sometimes follow comparatively slight injuries to the brain or its coverings.

Fred. Wilson, æt. 21, while out shooting on Christmas-day 1870, received a wound on the forehead from the bursting of his gun. I found him sitting quietly by the fireside, apparently not much the worse for the accident. There was a stellate wound right in the middle of the frontal bone about the size of a garden pea; there was little or no hæmorrhage, no pain, and, indeed, Wilson considered he had escaped any serious injury. On the introduction of a probe, however, into the wound, I found fracture of the bone, and, strange as it may appear, the probe glided easily and straightly up to its eye into the substance of the brain. I ordered him to keep quiet, gave him an aperient, and put him on milk diet. At the end of a week from receipt of injury, he seemed, and expressed himself as feeling, quite well. His pulse, though sometimes uncertain as to frequency, was otherwise natural, and there was neither pain in his head nor any abnormal increase of temperature. For two months he remained in this somewhat negative state, and, but for the presence of the small suppurating wound in his forehead, no one would have detected anything abnormal; nevertheless I thought it my duty to warn him and his friends as to the untoward results that might yet occur, the more so as he was anxious to "declare off" his sick-club and go to work. This he did not do, however; and ten weeks after the accident he began to complain of headache, slight sickness, and giddiness; and, by thermometer, I discovered irregular, and often extreme, elevations of temperature, and other general symptoms of brain-mischief. Day by day, from this time, these untoward symptoms were more decidedly pronounced, and with giddiness, exophthalmos, and enormously dilated pupils, the poor fellow gradually drifted into profound coma, and died just thirteen weeks after receipt of the injury. On opening the calvaria twenty-six hours after death, there was found a small canal leading antero-posteriorly from the frontal wound into an abscess. The latter was the size of an orange, and situated right above the right ventricle, the roof of which formed the floor of the abscess. There was a very thick pyogenic membrane of a dark ashy-gray colour, but no foreign body of any kind could be detected in the pus, which was thick and rather dirty-looking. The meninges in the neighbourhood of the abscess were rather thickened, but otherwise the brain and all its coverings were healthy.

ARTICLE V.—*On the Mechanism of Arrestment of Hæmorrhage in Cases of Placenta Prævia.* By J. MATTHEWS DUNCAN, M.D.

THE restraint or absolute stoppage of hæmorrhage in cases of placenta prævia may be produced in a variety of ways. The flow of blood through tubes of a peculiar shape is a pure mechanical phenomenon, and its arrest is also such. No doubt, in uterine floodings, feebleness of the heart's action, or actual fainting, will have a hæmostatic effect, but such influences have no special bearing on uterine floodings, and do not fall to be considered here. In cases of placenta prævia there is little that is peculiar in the arrestment of this flow, for accidental hæmorrhages are of course subject to, and also brought under, the direct influence of the same causes of arrestment as act on the former class of cases.

Among causes of arrestment have to be considered,—clots in the vessels; clots over the bleeding surfaces; the change of shape of the uterine sinuses; uterine action; local anemia; general anemia; diminution of, or absence of indirect blood-pressure; pressure by the entire ovum or by the fœtus; partial or complete, spontaneous or artificial, separation of the placenta; evacuation of liquor amnii; advance of labour.

The coagulation of blood in the maternal cells, or blood spaces, of the separated portion of placenta, is well known and recognised as the cause of the stoppage of the flow of blood through them from the corresponding cells of the still attached portion. This forms the peculiar thrombosis of a prematurely separated portion of the placenta; and the numerous foetal villi entangled in the clotted blood and having connexions with the walls of the maternal cells, give the coagulated blood a fixity and obstructing power which otherwise it would not possess.

In the so-called circular or marginal sinus of the placenta, I have never observed such a clot as would form anything like a complete obstruction to the passage of blood along the course of the vessel. Bleeding through this circular sinus may be arrested by complete separation of the placenta, or by thrombosis of all those portions of placenta from which the opened sinus gets its supplies of maternal blood. This will be sure to happen if these portions are detached; and it may also happen if they are attached. But bleeding from a portion of circular sinus may be arrested by complete separation of the portion. Its complete separation may no doubt lead to the opening of another portion of circular sinus unconnected with the former; but it may also leave the line of separation at a point where the sinus is absent, or where the new portion is uninjured.

In regard to the uterine sinuses, I am not aware of any observations which would lead us to suppose that clots of blood in their course are formed during labour. At their orifices, it is probable that clots are formed during labour, which may enter them, cork-

like, for a very short distance. Such cork-like clots may form an obstacle to the flow of blood from them, but I do not believe their resisting force is so great as to be ever of much consideration in the stoppage of bleeding.

In like manner, I regard the formation of a layer of coagulum on the detached surfaces of the uterus and placenta as not altogether unimportant in arresting or helping to arrest hæmorrhage, but also as exerting a very slight force in this way.

There is a general consent, and I believe justly so, that the divided curling arteries are not a source of ordinary uterine hæmorrhage, whether unavoidable or accidental. It is therefore unnecessary to enter upon the mode of arrestment of bleeding from them.

When the placenta is partially separated, the uterine sinuses corresponding to the detached portion will naturally be expected to collapse, on account of the failure of the current of blood, from the now unconnected portion of placenta, which formerly kept them more or less distended. When the blood-pressure is considerable, and especially in the absence of uterine action, they may no doubt be kept nearly as replete as formerly, or as the neighbouring sinuses which retain their direct connexion with the placenta, and in this case there will be hæmorrhage from them. This absence or imperfection of collapse may be to any degree, and the bleeding in proportion. But in most cases the collapse appears to be sufficient, as mere diminution of area, to completely or nearly completely arrest the passage of blood. Now, the sections of uterine sinuses are always elongated, spindle-shaped openings, whose increasing collapse will bring their two opposite sides nearer and nearer together, and in this respect diminish their area. The area of a section of a uterine sinus, in early labour in a case of placenta prævia, will be liable to this collapse, and it is not probable that the expansion of uterine wall will materially increase it by elongation. In any case, it may be regarded as certain, as we have elsewhere shown, that this change of shape, even supposing the area to be somewhat increased by elongation the result of uterine expansion, will have a very powerful influence in retarding or stopping the flow of blood through sinuses so collapsed or compressed. The less the amount of blood flowing through the sinuses, the more will they collapse. The more the uterine sinuses collapse, the greater resistance do they offer, by retarding friction, to the passage of blood through them.

It is in this way—namely, by collapse and change of shape—that the bleeding is arrested in cases of placenta prævia, while uterine action is absent and whether labour is going on or not. It is inconceivable that hæmorrhage should not go on if the sinuses remained as full as they were before the separation of the placenta. Did they remain thus filled, blood would certainly run from them freely. The uterine sinuses form an elephantine reticulation, the whole of

which may be filled, without regurgitation or reflux of blood, from any neighbouring sinus. The blood-pressure is, in cases of arrest of hæmorrhage, so slight in those sinuses connected with the placenta, through which blood is necessarily flowing, as not to force it into the sinuses corresponding to the separated portion, the change of shape in which makes such entrance of blood more difficult than it otherwise would be. In the same way is the arrest of bleeding accounted for, in cases of successful resort to Simpson's or Barnes's method of treatment, by total or partial separation of the placenta; for in such cases there is not necessarily uterine action, but only detachment and stoppage of the ordinary course of the circulation of the whole or of a portion of placenta in this region. The arrest of the bleeding in all cases of separation of the placenta, when the uterus is not in action, is to be explained in the same manner; among such are to be included the cases of which Simpson¹ has given an interesting account, where, in twin-pregnancy, the placenta of a born twin was separated and expelled before the birth of the other, where the placenta of a still unborn twin was separated and expelled before the placenta of an already born twin, and where both placenta were expelled after the birth of a first of twins. It is common, in order to account for this kind of stoppage of bleeding, to refer it to the formation of clots in the uterine sinuses; but it is plain that such clots will not be formed until the bleeding has already stopped.

Uterine-action will increase the change of shape favourable to the arrestment of bleeding, by compressing the uterine wall and its contained vessels between its own contracting tissue and the separated portion of placenta or unruptured sac of the ovum, or the adjacent foetal part. We do not say that such compression will stop the hæmorrhage flowing from the uterine sinuses, but it may do so, and it will certainly help towards this end.

Mere uterine action must have a direct hæmostatic effect, for it will make the bleeding area less than it otherwise would be, and it will so alter the area and shape of each open uterine sinus as partially or completely to arrest the passage of blood through it. Pains, or mere uterine action, have been very generally supposed to be a cause of hæmorrhage, but it is plain that they cannot be so directly, as I have elsewhere² attempted to show.

In cases of placenta prævia, there are frequently only moderate or even small gushes of blood, both before and after labour commences. It is both natural and proper to regard these bleedings as being the result of emptying of replete vessels, and as stopped by the production of local anæmia.

In great bleedings, general anæmia will be rapidly produced; and such general anæmia, being accompanied by great diminution of blood-pressure, will certainly lead to diminution or even to arrest of

¹ Selected Obstetrical Works, p. 229.

² British Medical Journal, November 1873.

flooding. When a woman has already lost large quantities of blood, there is now comparatively little to lose; and in such an exsanguine patient, a little, in mere measured quantity, may be equivalent, in vital importance, to very much while the original stock of blood was as yet entire, or drained away only to a slight extent. This circumstance, which endows the epithets, "little," or "slight," or "inconsiderable," with two meanings, one referring to measured quantity, the other to vital importance, renders the use of such terms a cause of ambiguity and of error. This mistake is well exemplified in authors who are over-zealously supporting any plan of treatment. Thus, we find them describing the results of their treatment as being the reduction of the bleeding to a now "slight" or "inconsiderable" amount, in complete forgetfulness that the patients' previous losses and previous danger were perhaps compatible only with such anæmia as rendered losses large in measured amount now impossible; and also rendered losses, small in measured amount, quite as great in vital importance as the grand gushes in the earlier history of the cases. In Simpson's well-known and laboriously-collected statistics of separation of the placenta before the birth of the child, we find this great mistake of confusing the two meanings of "slight" or "inconsiderable," and a consequent loss of value in the conclusions drawn from them.

Already, elsewhere,¹ I have directed attention to the influence of indirect blood-pressure, the result of absence of retentive power or adspiratory or sucking-in force in the abdomen, or the result of presence of a positive expulsive force in this cavity, as causing or favouring uterine hæmorrhage. Here I have only now to refer to the influence of a strong retentive power or adspiratory force in helping to stop or restrain uterine hæmorrhage. This force cannot prevent hæmorrhage while the placenta is being detached; but other hæmorrhage in connexion with placenta prævia, it may not only restrain or stop, but it may more truly and frequently be described as preventing. This subject² has been much discussed, especially by German obstetricians, and its laws have been made the subject of many special inquiries by Schatz.³ It is a very difficult and complicated matter, but already its practical importance has been proved in many different ways; and it may be expected that farther research will discover the means by which this retentive or adspiratory power is to be augmented with a view to favouring the arrest of uterine bleeding.

Many authors, among whom are Gendrin,⁴ Ramsbotham,⁵ Campbell,⁶ and Radford,⁷ regard pressure by the child's head as, when

¹ British Medical Journal, November 1873.

² See the Author's Researches in Obstetrics, p. 409.

³ Archiv für Gynækologie, 1872, etc.

⁴ Traité Philosophique de Médecine Pratique, tome ii. p. 275.

⁵ Observations on Midwifery, part ii. p. 191.

⁶ System of Midwifery, p. 369.

⁷ Provincial Medical and Surgical Journal, 22d January 1845.

duly applied, a means of stopping the flooding in cases of labour complicated with placenta prævia, and there can be no doubt that this good result will follow such application of pressure. At the same time, there can be as little doubt that these authors were very far mistaken in ascribing to such pressure, in actual cases, very much more influence than it has or can have. Such pressure, if required to stop the bleeding, can scarcely be efficient, except when pains are present, and hæmorrhage may go on in their absence. Such pressure cannot prevent the spontaneous premature detachment of the placenta, which is the real cause of hæmorrhage. Such pressure of a presenting head can act only during the early part of the first stage of labour, while spontaneous premature detachment is going on, and can therefore have no influence on the hæmorrhage which may persist after that time is past. Simpson¹ has ingeniously shown, so far as his statistics are of value, that great or considerable "hæmorrhage, after the expulsion or detachment of the placenta, is not more liable to occur when the retained fœtus afterwards presents by an upper or lower extremity, than when it comes down with the head or breech upon the exposed surface of the cervix uteri." Although these last words indicate an erroneous view of the source of the bleeding, yet the statistical conclusion is of interest and value. At the time when the head or breech is pressing upon the exposed surface of the cervix uteri, the real site of placental attachment may be pulled high up above such pressure. Barnes² also speaks of pressure on the vessels of the cervix, and refers to Legroux,³ who regards the erect position as having a certain influence in increasing this pressure by bringing the weight of the child to bear upon the bleeding surface. But these authors are very far wrong, forgetting the demonstrable fact, that the erect position will take the weight of the child off the supposed bleeding part, instead of bringing it to bear upon it. Had the erect position the effect erroneously imagined, it might be of use; but, so far as I can see, it must have in fact baneful results, and such only.

It is impossible to contemplate the sources of bleeding in placenta prævia without recognising the beneficial influence of pressure, however exerted, on the bleeding parts. Such pressure may, no doubt, be exerted slightly during the intervals of pains, more strongly during their continuance; and the medium of pressure may be the separated portion of placenta, the bag of waters, or some part of the child. Authors greatly mistake in regarding the cervix as the part to be pressed. That is not the bleeding part. They also mistake in regarding the lowest or presenting part of the child as the part pressing. The part of the ovum pressing, or that may press, the bleeding surface, is the presenting part of the child only in very

¹ Selected Obstetrical Works, p. 217.

² Lectures on Obstetric Operations, 2d edition, p. 420.

³ Archives Générales de Médecine, tome ii., 1855, p. 656.

early labour, and then, in most cases, only after rupture of the bag of membranes. If bleeding lasts, which it generally does not, after the first stage is well advanced, then the part of the child pressing, or that may press, on its source, is high above the presenting part, the height varying according to the pulling up of the internal os uteri, and the pushing down of the ovum. Both placenta and placental site retreat, as Gendrin very well knew, with the advance of the first stage of labour.

In my own practice, and in the records of experience in placenta prævia, I have not been able to make sure, at any particular time in any case, that pressure, of the kind we have just been discussing, was the direct cause of diminution or arrest of hæmorrhage; yet, as I have already said, when exerted, it must have a beneficial influence.

Spontaneous complete separation of the placenta when it is prævia is frequently accompanied or followed by arrest of hæmorrhage, frequently by diminution of its amount. The value of diminution in amount of hæmorrhage it is impossible to estimate nicely in consequence of our ignorance of the degree of the already existing and recently produced general anæmia, and in consequence of our at present possessing no good means of measuring this degree in any actual case. But the complete arrest or stoppage of the bleeding is a distinct statement, and one that may be verified. Now, the frequency of arrest of the bleeding by spontaneous separation of the placenta has been very much exaggerated by the enthusiastic Simpson. Nothing can be more unsatisfactory than the character of the statistical data on which he founds his opinion, that "in one only out of every twenty-two labours does there appear to have been a continuation of hæmorrhage to a great or profuse degree after the placenta was detached,"—and the statement is, in my opinion, of little value. If, however, we analyze his table of statistical data, as described by himself, we find only 47 cases fitted to throw light on the question of the arrest of hæmorrhage by complete separation of the placenta. These 47 were cases in which an interval, varying from ten hours to ten minutes, elapsed between the expulsion of the placenta and the birth of the child. In these 47, there was opportunity to judge whether bleeding was stopped or not. In the rest of his cases, as statistically arranged, the interval between the separation of the placenta and the birth of the child was either less than ten minutes, or it was not known; and in the former of these two last categories there was not a sufficient lapse of time to allow of secure observation of the effect of the separation. Now, in the 47 cases, where there was a sufficient interval for this observation, I find that in 25 cases only, or in little more than one in two, was the bleeding arrested. In all the rest it appears to have continued in a greater or less degree. But this numerical statement is also of little value, in consequence of the unsatisfactory character of the data in every respect.

In cases in which complete spontaneous separation of the placenta takes place, and in which the bleeding is arrested, it has to be asked, Is the stoppage of the bleeding the consequence of the complete separation of the placenta? I can see no reason whatever for thinking so. Were the bleeding through the placenta, having for its source the open utero-placental vessels in the placental surface, then the bleeding should always be arrested by complete separation. We have just seen that this is far from being the case, for we find from Simpson's great table, that it continued in nearly one half of the cases in which spontaneous separation took place. But in addition to this, we must recall the general belief of obstetric pathologists, which we have endeavoured to confirm, that the bleeding in placenta prævia is not through the placenta. The bleeding, which, in nearly one half of the cases, continued after complete separation, must have come from the uterine site of the detached placenta. Bleeding from the so-called circular sinus is of course arrested by complete separation; but bleeding in nearly one half of the cases persisted after the separation, and the stoppage of the placental source did not stop the bleeding, which flowed on in greater or less quantity from its chief origin, the open vessels on the placental site.

But, if the frequent arrest of the bleeding after complete separation is not accounted for by the detachment of the placenta, how is it explained? Now, it appears to me to be so natural a result of the circumstances as scarcely to require explanation, and the causes of it have already been or will hereafter be fully considered. Meantime, I may briefly state those not already discussed. Spontaneous separation of the placenta is indisputable evidence of energy of labour and of great progress of the first stage. Now, it is well known that these two conditions do in any case generally arrest the flooding, and Barnes has contributed greatly to the explanation of why it should be so. It is only in certain cases, chiefly of central or nearly central attachment, that the placenta is spontaneously prematurely separated entirely. In the majority of such cases, if the labour goes on with energy and rapidity, no further hæmorrhage of importance is to be expected, whether the placenta be completely detached or not. In short, complete spontaneous separation of the placenta indicates such characters in a labour as would lead us to expect early arrest of the bleeding, apart entirely from the remarkable circumstance of the complete separation.

In itself, complete separation of the placenta must have an injurious tendency, if there is any truth in the views universally entertained as to the source of the hæmorrhage, for it increases the bleeding area to the greatest possible extent. The evil effects to be expected from it are, no doubt, to a great extent averted by the characters of the labour which produce it. If in any case there were a great tendency to uterine hæmorrhage, direct or indirect blood-pressure being great, nothing but strong and rapid labour could

save the woman from the evil effects of extension of the bleeding area. Fortunately, in most cases of placenta prævia there is no great mechanical tendency to uterine hæmorrhage, for contraction of the uterus cannot be relied on to check it so long as the child is unborn; and some authors have erroneously supposed that it can.¹

Artificial premature detachment of the whole placenta I shall not here discuss, because, with some change, most of the remarks made on the spontaneous premature total detachment apply to it as well. Besides, I have no good collection of data whence to derive a satisfactory basis for remarks. At the same time I add, that I have no doubt that artificial premature separation is frequently followed by arrestment of the bleeding; for, as already said, most cases of placenta prævia have little tendency to bleed after the unavoidable amount of separation is effected.

Artificial partial detachment of the placenta is resorted to in the treatments of Cohen and of Barnes, and in both, apparently, with considerable effect in arresting or diminishing the hæmorrhage. The method of Barnes acts very much as complete separation does, and is liable to nearly the same criticism. In it, as in complete artificial separation, the same amount of benefit cannot be expected as follows when the partial and complete separation are spontaneous, and for the same reasons. Spontaneous partial separation shows energy and progress of labour, which are not ensured by artificial partial separation. Artificial complete separation has the advantage over partial separation, whether spontaneous or artificial, that the possible bleeding during partial detachment, from a utero-placental sinus, is rendered impossible. But, apart from mere therapeutical considerations, which I am not here considering, the method of Barnes has the advantage, in theory at least, of not extending the bleeding area beyond what it must necessarily reach as labour advances. The method of Barnes stops, or rather unites into one, all the gushes from the renewed partial separations as labour advances, and only those gushes are so united which must necessarily take place as labour advances; whereas the complete separation, even if successful, will have, during its artificial production, a greater amount of gushes united than were inevitable or possible. The method of Barnes does for a time, or prematurely, increase the bleeding area above what it would otherwise be; not, in theory at least, above what it would ultimately be as the first stage of labour progressed. In this respect it is far less injurious than the method of complete separation; and it is possible that even this evil may be counterbalanced by the advantage of having united in one all the bleedings consequent on renewed partial separations; for such single gush will probably be less in amount than the united gushes of repeated separations of parts, which, taken together, constitute an amount of separation equal to that represented by such single gush. Against this possible advantage

¹ See Barnes's "Lectures on Obstetric Operations," 2d edit., p. 404.

there has, however, to be placed the possible great disadvantage of extending the bleeding area earlier than is necessary in those cases in which there is a great, or even a slight tendency to persistent bleeding. Most cases have, no doubt, no such tendency when the partial separation of Barnes is completed; but there is reason to believe that the cases requiring greatest consideration and energy in treatment are those in which the partial separation, according to Barnes's plan, has not the usual or desired effect. For, as this author truly remarks, though with a very different application, "there is no independent virtue in the mere detachment of the placenta, as *post-partum* hæmorrhage abundantly proves."

The method of Simpson and that of Barnes frequently conduct to arrest of bleeding, but far from invariably, as both authors admit. Their methods do not nearly always stop the bleeding. They both prematurely extend the area fitted to discharge blood.

This is not the place for trying to estimate their value as therapeutic measures, but we may quote Barnes's statement that, "when the dilatation of the cervix has reached the stage at which the head can pass, and all that part of the placenta which had been originally adherent within the cervical zone is detached; and if, as is the constant tendency of nature to effect, the intermitting active uterine contractions arrest the hæmorrhage, a stage is reached when the labour is freed from all prævia placental complication; the lateral portion of placenta retains its connexion, supporting the child's life; the labour is, in all respects, a natural labour."¹ This allegation as to labour being, in such circumstances, in all respects natural, is a misleading rhetorical trope, not justifiable in a scientific work, but which the generous enthusiasm of the author may be allowed to excuse.

It is a great mistake to suppose that "the one constant condition" of arrest of bleeding, in any circumstances, "is contraction, active or tonic, of the muscular structure of the uterus."² Very many cases, indeed, of arrest of bleeding occur quite independently of uterine action. Were it not so, there could be little advantage expected from artificial separation of the placenta, whether complete or partial; for these means do not necessarily imply uterine action. But we have already shown that uterine action is always a restrainer of hæmorrhage, if not a stopper.

Although it has no reference to placenta prævia, yet the following passage from Dr R. Ferguson illustrates the argument of the previous paragraph, and we therefore quote it. In doing so we do not mean to admit that plugging of the veins by coagula is the only or even the chief means of arrest of hæmorrhage in the cases referred to. We merely wish to illustrate absence or arrest of hæmorrhage without action or tonic contraction of the muscular structure of the uterus:—"The latter (plugging of the veins by coagula) appears

¹ Barnes, Lectures on Obstetric Operations, 2d edition, p. 408.

² Ibid., p. 421.

to be the sole means of safety," says Ferguson, "in those cases of intense flooding, in which the uterus flaps about the hand like a wet towel. Incapable of contraction for hours, yet ceasing to ooze out a drop of blood, there is nothing apparently between life and death but a few soft coagula plugging up the sinuses."¹

Uterine action during labour has two direct results: first, the ordinary intermitting efforts, which perform the chief part of the mechanical work of labour, and whose influence in arresting hæmorrhage has already been discussed; second, the gradual progressive decrease of capacity of the organ, progressing from its state of repletion, when labour begins, till its state of emptiness, when complete delivery is effected.

Now, the whole of this second progress does not imply persistent active or tonic contraction of the uterus, but it necessarily involves diminution of the area of its surface, and will be, from first to last, favourable to the diminution or arrestment of bleeding. It cannot advance without diminution of the bleeding area below what it would be were this progressive shrinking absent.

The evacuation of the liquor amnii, whether spontaneous or artificial, is a great means of contributing to this progressive uterine shrinking, and this is the chief part of the explanation of its hæmostatic influence; but it may also contribute to hæmostasis indirectly by increasing active uterine action or pains. The evacuation of liquor amnii, then, and the advance of labour, both offer ever-increasing security against the persistence or recurrence of hæmorrhage, and the influence of both is easily explained and understood.

This is so well stated by Gendrin, that we quote his words,² although we do not altogether agree with his remarks on uterine inertia. "*Dans les cas où les eaux se sont écoulées peu à peu par une rupture des membranes opérée dans l'utérus, quelle que soit la partie du placenta qui réponde à l'orifice du col, l'hémorrhagie ne se montre plus que dans les intervalles des contractions utérines, et encore dans ces cas est elle toujours peu considérable, et ne devient-elle véritablement alarmante que lorsque la matrice tombe dans l'inertie. Si les contractions sont efficaces et si l'enfant s'engage, l'hémorrhagie se suspend définitivement. Nous avons constaté cette suspension de l'hémorrhagie dans tous les cas de cette espèce dont nous avons été témoin; Rigby l'avait déjà remarquée; elle est si efficacement produite par l'accomplissement réel du travail expulsif du fœtus, que nous venons de l'observer dans un cas où le placenta était inséré par son centre sur l'orifice du col, et a été entraîné comme une calotte sur la tête de l'enfant, et cependant le placenta avait été déchiré profondément et en divers sens, en s'engageant dans le col sous l'influence de la pression qu'exerçait sur lui postérieurement la tête du fœtus.*"

¹ See page xlii. of Introduction to the New Sydenham Society's edition of "Gooch on some of the most important Diseases peculiar to Women."

² *Traité Philosophique de Médecine Pratique*, tome ii. pp. 197 and 347.

"La suppression de l'hémorrhagie utéro-placentaire par implantation du placenta sur le col de l'utérus, arrive toujours dans les cas d'implantation par un segment du placenta dès que les contractions utérines expulsives de l'accouchement s'établissent. Dans les cas d'implantation centrale, l'hémorrhagie, augmentée par les premières contractions, cesse dès que le travail d'expulsion a déterminé l'évacuation des eaux de l'amnios; l'inertie seule de l'utérus peut la faire recommencer. Nous sommes étonné que la plupart des auteurs qui ont traité de l'art des accouchements n'aient pas fait cette remarque, et surtout que quelques uns aient été jusqu'à regarder cette suppression comme un fait rare dans ces cas. Si le sang jaillit du col de l'utérus à chaque douleur utérine, ce n'est que pendant la première période de l'accouchement, et jusqu'à ce que les contractions utérines soient franchement établies pour les cas d'implantation latérale du placenta; ce n'est que dans les cas d'implantation centrale que cette augmentation de l'hémorrhagie continue jusqu'à l'évacuation des eaux de l'amnios. Les choses se sont ainsi passées dans tous les cas d'hémorrhagie utéro-placentaire dont nous avons été témoin. Nous regardons donc comme une circonstance toujours heureuse l'établissement du travail de la parturition dans les cas d'hémorrhagie utéro-placentaire par implantation anormale du placenta. Nous en déduisons comme conséquence l'indication de déterminer artificiellement l'accouchement dès que ces hémorrhagies, par leur abondance ou leurs fréquentes répétitions, menacent la vie de la femme."

At last, in cases of placenta prævia, as also in cases of accidental hæmorrhage, the stage of complete delivery is reached. All danger of hæmorrhage necessarily connected with the premature separation of the placenta is passed. If hæmorrhage occurs now from the placental site, it is post-partum hæmorrhage. If the uterus is firmly contracted, hæmorrhage from the placental site does not occur.

But eminent obstetricians have tried to unsettle the professional faith in a contracted uterus as an efficient hæmostatic after delivery. It is the sagacious Gooch¹ who is responsible for this belief, that flooding may come in the usual way from a uterus well contracted after delivery. Gooch founds his belief on a case whose history he gives; and, remarkable to tell, this case presents nothing at all unusual. It is just a case of ordinary flooding post-partum, such as every accoucheur is familiar with. After delivery, the uterus became well contracted, but there was no flooding from it then. Subsequently, it became relaxed, and flooding began, as Robertson² has pointed out; and that it was so relaxed is proved by the fact that Gooch put his hand into it, in order to induce contraction and

¹ See his Essay on "A Peculiar Form of Hæmorrhage from the Uterus," in his collected "Essays on some of the most Important Diseases peculiar to Women."

² Physiology and Diseases of Women, p. 362.

stop the hæmorrhage!!! Gooch had, in fact, no ground whatever for unsettling the general faith of the profession in a contracted uterus after complete delivery. In my opinion, this article of medical faith is unassailable.

But Gooch has had many followers, among whom are Velpeau,¹ Rigby,² Ingleby,³ Michælis,⁴ Ferguson,⁵ and Farre;⁶ and against whom Boivin,⁷ Robertson, and Barnes,⁸ have given evidence.

In order to substantiate the belief of Gooch, it will be necessary to have cases carefully observed by a competent person who is well aware of the various sources of error in judgment. I know of no such cases.

It has been adduced as telling in favour of Gooch's view, that copious hæmorrhage may take place from an unimpregnated uterus. But such hæmorrhage is not post-partum flooding. It is not from the same source. Besides, we cannot tell whether in such cases the organ was in a state of firm contraction or not. Similar remarks may be made in answer to any defence of Gooch's doctrine, founded on the frequently fatal hæmorrhage of uterine fibroids.

The proposition I enunciate is, that it has never been shown that uterine hæmorrhage, of the ordinary post-partum kind, ever takes place from the uterus when it is in a state of moderately firm general contraction.

Among the sources of error are the following:—

1. Variations in bulk of the empty and contracted uterus. If an observer has a preconceived idea of the bulk of the empty uterus taken from the ordinary run of cases, he may, in an individual case, if judging from size, be greatly deceived. For some uteri are extremely small (and were no doubt proportionally weak) after delivery, scarcely larger than the fist; and such a uterus, if expanded to an ordinary size, would certainly be in a state of relaxation. Other uteri are, after delivery, and when in strong contraction, very large when compared with the ordinary size (and were, no doubt, proportionally strong).

2. Variations in position of the uterus after delivery. If the uterus has sunk into the true pelvis, it may appear small and contracted when it is merely collapsed and depressed in the abdomen. If the uterus is high in the abdomen, which is often a consequence of repletion of the bladder, it may appear to be larger than it really is.

3. A uterus may be hard and feel as if contracted, when the hardness is merely the result of passive tension. This is often seen

¹ *Traité complet de l'Art des Accouch.* Ed.: Bruxelles, 1835, p. 489.

² *System of Midwifery*, p. 218.

³ *A Practical Treatise on Uterine Hæmorrhage*, p. 227.

⁴ See Rigby, loc. cit.

⁵ Introduction to the New Sydenham Society's edition of Gooch on some of the most important Diseases peculiar to Women, p. xli.

⁶ See Ferguson, loc. cit.

⁷ See Velpeau, loc. cit.

⁸ *Lectures on Obstetric Operations*, 2d edit., p. 455.

in cases of the kind called hour-glass contraction. On this subject I have elsewhere remarked as follows :¹—"In their descriptions of, and reasonings upon, cases of hour-glass contraction, of inversion, and of hæmorrhage, many authors have erred in taking a hard part of the uterus for a part necessarily in a state of action. Now, while hardness of uterine wall is in most cases a good and sufficient sign of muscular action in the hardened part, it is not invariably so. For, in cases of hour-glass contraction and of inversion, the hardness may arise from passive tension. The bladder of urine and ovarian cysts frequently illustrate hardness of flaccid bags produced by passive distension. In like manner, a paralyzed portion of uterus may be hard from being tightly replete with placenta or other contents retained by stricture; and the fundus of an inverted uterus may be hardened by the circumstances of its inversion, apart from muscular action of the hard part."

4. Great and even fatal hæmorrhage may be erroneously supposed to be ordinary post-partum flooding, when it is really running from an unsuspected source. I have seen an artery in the perineum bleeding freely twelve hours after delivery. Among such unsuspected sources are injuries of the cervix, of the vagina, of the perineum, of the nymphæ, aneurismal bursting, so-called varicose hæmorrhage, uterine hæmatocele, etc.

Part Second.

REVIEWS.

WORKS ON DISEASES OF THE EYE.

1. *On the Use of the Ophthalmoscope in Diseases of the Nervous System and of the Kidneys; also in certain other General Disorders.* By THOMAS CLIFFORD ALLBUTT, M.A., M.D. Cantab., etc. London and New York: Macmillan and Co.: 1871. Pp. 405.
2. *A Manual of the Diseases of the Eye.* By C. MACNAMARA, Surgeon to the Chaudine and the Ophthalmic Hospital, Calcutta, Professor of Ophthalmic Medicine and Surgery in the Calcutta Medical College. Second Edition. London: J. and A. Churchill. Pp. 620.
3. *Three Lectures on Preservation of Sight.* By DAVID SMITH, M.D., M.R.C.S.E., Extra-Academical Lecturer on the Eye, Glasgow, etc. London: Robert Hardwicke: 1871. Pp. 92.

WE really owe an apology to our readers as well as to the author for having so long delayed our notice of Dr Allbutt's excellent

¹ Researches in Obstetrics, p. 392.

work. The subjects therein discussed are of such importance and interest as to be worthy of the attention of all medical practitioners. As the title of the book indicates, the author's aim has been to point out the light that may be thrown on the nature, seat, or course of certain affections of the general system, by the appearances presented on examination of the interior of the eye by means of the ophthalmoscope. The manner in which Dr Allbutt has fulfilled his task is worthy of all commendation. He has not merely recorded his own experience, but has also carefully embodied the views and observations of most others who have written on the various subjects discussed. Much of the contents of the present volume appeared previously in the form of articles contributed to various medical journals; but all this material has been revised, and in some parts entirely remodelled. As it at present stands, it is, as far as we are aware, the only work in which this intricate subject has been in anything like a complete manner grappled with.

Dr Allbutt, after describing the method of using the ophthalmoscope, and the natural appearance of the retina and extremity of the optic nerve ("the optic disc"), considers the variations from health observed in these textures. The theory that the degree of injection of the optic disc and retina is regulated by the cervical sympathetic is vigorously controverted, as the following short extract indicates:—

"It is very frequently stated, and stated with a confidence which is quite unjustified by any complete or accurate observation, that the sympathetic nerve-system is endowed with great power over the vascularity and nutrition of the optic disc and retina. Fancies about the sympathetic nerves are now very fashionable, and their mysterious agency is called upon every week, every month, and every quarter, to explain all sorts of phenomena, or supposed phenomena, in healthy physiology, morbid physiology, and therapeutics. The less the writer and the reader know about the sympathetic nerve-system, the more satisfactory, of course, is the explanation. It is asserted in most ophthalmic treatises that paralysis of the sympathetic in the neck causes hyperæmia of the optic disc and retina. This may be so: indeed, I am far from saying that it is improbable; but, so far as I know, it is wholly unproven; nor do I find any proof of this assertion in the writings of those who repeat it. Bernard certainly says nothing of the kind; and in cases where I have been able to examine the back of the eye during conditions of disorder in the cervical sympathetic, I have found no consequent hyperæmia. I have examined the eye carefully with the ophthalmoscope in two cases of aneurismal pressure upon the cervical sympathetic, in three cases in which the cervical sympathetic was paralyzed by disease of the neck, in many cases of Graves's disease, in which latter disease the sympathetic is said to be at fault, and in many cases submitted to galvanism of the sympathetic in the neck; but I have obtained

no constant results. There is sometimes *venous* hyperæmia in Graves's disease; but this I should refer, as I have said, to venous obstruction rather than to the paralysis of the carotid. Few conclusions command my wonder more than the common accusation of the sympathetic nerves in the so-called 'spinal amaurosis.' If the sympathetic can set up an atrophic process, no doubt it may easily establish hyperæmia; but I have yet to learn that there is a single valuable clinical fact to be adduced in favour of either process."

Somewhat corroborative of Dr Allbutt's views is the fact that the inhalation of nitrite of amyl produces no marked change in the fulness of the retinal vessels, although the face be flushed, the ears tingle, and the carotids pulsate strongly (symptoms best explained in our opinion by a paralyzed condition of the cervical sympathetic). This, at least, was our experience when we investigated this subject, in the hope of finding in nitrite of amyl an agent whereby we could readily distinguish between simple anæmia of the optic disc and retina, and atrophic states of these tissues. We had hoped to find from the inhalation a normal degree of injection restored in anæmic cases, and but little effect produced where atrophy was present; but in these conditions, as in the normal, we could trace no change in the fulness of the vessels produced by the nitrite of amyl.

We are sorry we cannot agree with Dr Allbutt in believing that the condition of ischemia of the discs ("choked disc," or "stauungspapilla") is distinguishable by means of the ophthalmoscope from true optic neuritis. In both, we have swelling and projection of the disc, and increased injection of the vessels of retina and optic nerve; in both, slight extravasations of blood occur; and in both the disc and immediately adjacent retina appear of a reddish gray colour. The progress of the disease, too, is in both instances similar, and both usually terminate in atrophy of the nerve. What is described as ischemia is supposed to be caused by an obstruction to the return of the blood by the retinal vein, such as might be artificially induced by the application of a ligature to the ophthalmic vein; but if such were really the nature of the affection, we might expect the whole of the retina to be the seat of serous transudation from the veins, and not merely, as really occurs, that portion of the retina immediately surrounding the optic disc. We would also not expect to find increased growth of connective tissue and solid exudative material poured out, both of which pathological conditions microscopic examination of such cases has demonstrated. Post-mortem examination alone can reveal which of the conditions is present, the only true distinction being that in ischemia the inflammation is confined to the distal extremity of the nerve, while in neuritis a greater or less extent of nervous trunk is affected. We believe that ophthalmoscopically the two affections cannot be distinguished, varying merely in severity: where the changes are

most marked, the case is termed one of ischemia; where less marked, optic neuritis.

Dr Allbutt describes a chronic form of neuritis which we are accustomed to view as the early congestive stage of simple progressive atrophy of the optic nerve. Possibly the condition may be correctly viewed as one of chronic inflammatory action, but further pathological investigation is necessary to substantiate this view. The description of atrophy of the optic nerve (consecutive and primary) is, in our opinion, correct, concise, and in every way excellent.

Having discussed the appearance and nature of the diseased conditions of the optic nerve and retina usually met with, the remaining chapters are devoted to the consideration (specially with the view to diagnosis) of those affections of the encephalon and system generally, which usually occasion changes in the interior of the eye, recognisable by means of the ophthalmoscope.

Were we inclined to be critical of a work altogether so valuable, we might suggest that Dr Allbutt has attempted to carry too far, in some respects, the employment of his instrument, both in regard to the diagnosis of certain forms of intracranial inflammation, and in regard to localization of new formations. If we could corroborate our author's statement with regard to the distinctness and distinguishableness of the choked disc and optic neuritis, we might accept some of his conclusions; but this we have been unable to do. We would in actual practice trust more to the excellent résumé of the general symptoms with which he furnishes the reader. We, however, by no means desire to underestimate the value of optic neuritis (or ischemia) as a diagnostic symptom of the presence of serious organic disease of the encephalon. On the contrary, we are of opinion that double optic neuritis very rarely, if ever, exists, except as the result of serious encephalic changes; and, on the other hand, we would be inclined to doubt the accuracy of a diagnosis of cerebral tumour where optic neuritis was absent. The following case we commend to the attention of our readers, as an example of the advantages to be gained by examination with the ophthalmoscope in doubtful cases; adding only, that it has fallen to our lot also, on more than one occasion, to relieve intense anxiety in cases in which cerebral tumour was strongly indicated by the general symptoms:—

"Mrs G. was sent to Leeds by Dr Beaumont of Harrogate to be under my care. She presented many symptoms of tumour of the base. She suffered from intense paroxysmal headache, vomiting, and diplopia; there was also slight ptosis, and external strabismus of the left eye. These symptoms had lasted about six months. As the discs were absolutely healthy, I told Dr Beaumont that I felt almost certain there was no tumour of the middle fossa, and that I thought the absence of neuro-retinitis during so long a time should decide us also against syphilitic meningitis. Nor could we

discover any other evidence of syphilis; but still it seemed desirable to give trial to a full and prolonged course of iodide of potassium. This was done with no benefit whatever. As the paralytic symptoms were but slightly pronounced, and varied somewhat with the headache, I ventured to side with the ophthalmoscope, and to pronounce against the supposed presence of any organic disease. Under the care of Dr Beaumont, and the bold but watchful use of tonic remedies, this lady quite recovered, and has now been in good health for some time."

We value highly the account which our author gives of the eye symptoms in spinal disease, especially for the able manner in which he discusses the causation of amaurosis in spinal disease. He very thoroughly refutes the views entertained by Mr Wharton Jones, who attributes the atrophy of the optic nerve to changes in the nutrition of the eye, caused by the sympathetic nerve or its origins being involved in the spinal affection. It is well to overturn a false theory, even though we have no true theory to take its place; and we agree with the statement of Dr Allbutt, that "there remains much to be done before we can reason on spinal amaurosis and the ophthalmic signs of spinal injury in any adequate way." Only incidental reference is made to a contracted state of the pupils as present in spinal disease—a symptom we consider worthy of more attention.

It is impossible, in a short review like this, to do more than refer to the chapters on Albuminuric, Leukæmic, and Syphilitic Retinitis; on Diabetic and Toxic Amaurosis, etc., as being most instructive and worthy of attentive study. We must, however, express our dissent from the author when he questions the existence of tobacco amaurosis. The arguments he adduces, as disproving the connexion between abuse of tobacco and defective vision, would equally apply to the amaurosis from alcohol poisoning, the existence of which Dr Allbutt admits. Thus, the fact that the German oculists speak doubtfully of this effect of tobacco is, in our opinion, counterbalanced by the large proportion of cases of amaurosis (considering the temperate habits of the people) they attribute to abuse of alcohol. In the large majority of cases both poisons are at work, as most heavy smokers in this country also drink in excess. Where we think observers in this country have erred is in attempting to prove that certain ophthalmoscopic appearances are distinctive of tobacco amaurosis. This we have never been able to convince ourselves of. We base our belief in tobacco amaurosis chiefly on the three following grounds:—1st, In persons affected with this form of amaurosis, we frequently find other symptoms of nervous derangement most readily explained by the action of tobacco; 2d, In many cases there is absence of any other sufficient cause; and, 3d, The improvement resulting from the discontinuance of tobacco. A most illustrative case occurred in our own practice. A very heavy smoker came complaining of such dimness of sight, that he could read only the

largest type (No. 20 Jæger). After several months' abstinence from tobacco, he was able to read the finest print; but, unable to resist the seductive charms of his pipe, he in a short time reproduced his amaurosis.

The Appendix is almost as important as a lady's postscript, containing short notes of above 120 illustrative cases, and the tabulated results of the ophthalmoscopic examination of a large number of insane patients.

We expected much, both in the way of instruction and interest, from the perusal of this book, and we have not been disappointed. It justly adds to the deservedly high reputation of the author.

2. In the number of this Journal for March 1870, we reviewed the first edition of Mr Macnamara's work. Most of the errors we then pointed out have been corrected, and the suggestions we made adopted; and we have now no hesitation in expressing a more favourable opinion of the trustworthiness of the manual. We, however, regret to find the author still wedded to the view that the lens is a muscular structure, and that he still recommends the very large doses of opium in the treatment of granular conjunctivitis to which we previously took exception. We are not aware of any affection of the eye for which we would be inclined to order *two grains of opium three times a day*.

The illustrative plates have been reduced in number from fifteen to seven, the poorer plates having been eliminated; while the substitution of Suellen's for Jæger's types is a decided improvement.

By the free use of extracts from standard works on such subjects as affections of accommodation and refraction, paralysis of the ocular muscles, etc., accuracy is secured; but in the work of a professor of ophthalmic medicine and surgery, we would desiderate more original composition. On most points, we can recommend this book as a reliable guide.

3. Dr Smith's brochure was written for the instruction of parents, schoolmasters, students, and those who need spectacles, and is thus necessarily somewhat elementary in its scope. It is chiefly occupied with a clear and concise description of the physiology of vision, of the affections of refraction and accommodation of the eye, and of the various kinds of spectacles adapted for the relief of defective sight due to abnormalities of refraction. Although, as already stated, chiefly intended for lay readers, members of the profession may obtain much instruction and get many useful hints from a careful perusal of this little work.

Photographic Clinique of the British Hospital for Diseases of the Skin. A Quarterly Periodical. Edited by BALMANNO SQUIRE, M.B., F.L.S., Surgeon to the Hospital.

Segnius irritant animos demissa per aurem.
Quam quæ sunt oculos subjecta fidelibus.

London, W. Nos. 2, 3, and 4.

No. 2 illustrates Rodent Ulcer. It is an exceedingly good photograph, coloured at diseased part, of a tolerably typical case. The history is given pretty fully, one illustrating the unfortunate effect of spasmodically heroic local treatment by caustics. The photograph shows most beautifully the effect produced by the ulcer on the surrounding parts; the lower eyelid drawn down and slightly everted, the lip pulled up and stiffened, and all without the exaggeration and striving for effect, so common in even the best sketches.

No. 3 is a characteristic example of a serpiginous ulcerating syphilide of the back and shoulders in a man aged 68. The cicatrices of a previous attack are excellently shown, and the colour and texture of the ulcers and the circumference of scabs wonderfully imitated.

No. 4 is a wonderfully beautiful (artistically) picture of a most hideous deformity, so rare as to be the only case Mr Squire has ever seen of vascular nævus complicated with the hypertrophic form of acne. Interesting as a record of a very rare disease, this plate is not so practically valuable to the profession as delineations of more common maladies would be.

We have never seen anything more perfect or lifelike of their kind than those coloured photographs by Mr Squire. The touching up is so delicate, not overdone nor exaggerated. For purposes of clinical instruction such plates are invaluable. We hope that he will be encouraged by a large sale to continue the series, and particularly to illustrate some of the more common everyday diseases of the skin. The letterpress attached to each plate is brief, clear, and practical, just what is wanted, and no more.

A Manual of the Operations of Surgery, for the Use of Senior Students, House Surgeons, and Junior Practitioners. By JOSEPH BELL, F.R.C.S. Edin. Third Edition, revised and enlarged.

WHEN any work in surgery has reached its third edition, it has quite overpast that period of its existence when the reviewer's labours are necessary or advantageous. It has taken root among the class best qualified to judge of its merits, and any remarks

complimentary to its author, or in the way of advice for his guidance in the editions of the future, must be singularly impertinent and ill-judged.

We would only congratulate Mr Bell upon his well-merited success. The demand for the work among those for whom it is written proves that a most discerning and discriminating part of the profession attest its value. We are aware, however, that senior practitioners as well find a ready source of information in its pages, and are gratified by the simple pithy practical form in which the facts are arranged, and theoretical considerations weighed and decided upon.

The revision of the work has been efficiently conducted, and what additions have been made have been selected judiciously, not to swell the work into new proportions, but to bring it up to the advance of surgery at the present day. There is one striking feature in the book which requires notice; while it deals out to others, with even hand, credit where credit is due, the author entirely ignores himself, and, rare virtue, seems to require none of the egotistical pronouns which are so abundantly scattered through the pages of the medical and surgical writers of the day.

We wish for this third edition a continuance of the well-deserved success of the two former issues.

The Principles and Practice of Surgery. By WILLIAM PIRRIE, F.R.S.E., Professor of Surgery in the University of Aberdeen, Senior Surgeon to the Royal Infirmary, etc. Third Edition. With 490 Wood Engravings. Pp. 978. London: J. and A. Churchill: 1873.

It may be thought almost beyond our province to review the third edition of a work so well known as this is; one too, which, as the text-book of the Aberdeen School, has its own public and its own reputation. This edition, however, has been so thoroughly rewritten and altered as to be, in fact, almost a new work; and, besides, it is not so well known in Edinburgh as it is in the north. The author puts before himself the following aims:—"1st, To render a faithful account of the present state of surgical science and art; 2d, To arrange the subject-matter as simply as I could; and, 3d, To combine conciseness with clearness of expression." High aims, indeed—much higher than the mere writing of a text-book for a course of lectures; more ambitious, if less interesting, than a simple statement of one's own views on surgical pathology and treatment—almost an impossible achievement in these days of change and transition.

Tried by his own standard, we wish we could say the author had succeeded. The subject-matter is indeed arranged simply—with such an ingenuous simplicity, that we fail to see any principle

arrangement at all, either scientific, regional, or even alphabetical. Many amusing examples of these might be given; suffice it to say, that clubfoot comes in between curvatures of the spine and collapse, while amputations and resections separate diseases of the testicle from affections of the genito-urinary organs; and we seek for affections of the breast in vain till we find them between affections of the ear and affections of the integument, skin, tendons, and bursæ. Then, for conciseness and clearness, *non cuivis contigit*; still we would not have chosen these epithets to describe the majestic march of some of the rounded periods, or the cautious complication of much of the pathological teaching.

It is clear that the author has read an immense deal about, or even of, other men's work in pathology, but it is not clear that he has for himself formed any distinct idea whatever, and he has certainly failed to convey to others what his own views are. As examples of this, we may refer to the chapter on the inflammatory process, in which the old and the new, active hyperæmia, functional activity, inhibitory nerves, protoplasm, and leucocytes, are heaped together with a profusion of learning and an absence of teaching powers hardly to be expected in so highly respected and so old a professor; also, to the arrangement and nomenclature of ulcers, and to a wonderful chapter on the six varieties of erysipelas.

From a student's point of view, we fear that this book is rather a difficult one, hard to read, as, though the actual descriptions of diseases are fluent enough, there is a want of preciseness, and, still worse, a want of accentuation, i.e., the salient points which distinguish diseases are not carefully differentiated from the more common characters, and no care is taken to show in what the differences of various forms of disease consist.

Yet, after all we have said, the work is a valuable and honest one. There is much erudition, much zeal and love of the surgical art, the outcome of a long and useful life, both as an hospital surgeon and a teacher of surgery; and the educated surgeon will find in it much to interest, and more to instruct him. The practical part of the work is of a very much higher order than that which treats of principles or pathology. Dr Pirrie is a successful and learned surgeon, and yet still one who can be taught, and can receive and digest new ideas and new modes of treatment; consequently the practical portion of this book is well up to date. Bigelow's views on the hip-joint receive full attention. American instruments for reduction of dislocations are described and recommended. Acupressure received in Aberdeen not only by far the best trial and strongest evidence in its favour it ever received, and in this edition it is well described and still recommended. The carbolic acid method of opening abscesses, Dr Pirrie dismisses with the following sentence, as giving the result of his experience:—"This method is adopted by those surgeons who regard germs in the atmosphere as the cause of most of the evils which follow in

wounds, operations, or the opening of abscesses. The conclusion I have arrived at is, that, as regards abscesses, the carbolic acid treatment is not more successful than the method of opening described without it."—P. 61.

The description of acupressure is valuable, as conveying the experience of one who has used it so long. The seven methods of acupressure previously described are now reduced by Dr Pirrie to three, for which he has devised the following names: 1. Circumclusion. This requires a needle to be passed below the artery, and a loop of inelastic iron-wire over the point of the needle above the artery, and then twisted round the needle to the other side of the artery. 2. Torsoclusion, in which one pin only is used, and by which the artery is twisted and compressed. 3. Retroclusion resembles the second in the agent, differs in the amount of twist given. Though few surgeons nowadays dream of using acupressure, it is theoretically an unexceptionable mode of arresting hæmorrhage, and does not make progress, simply from the many practical difficulties and "botherations" it implies, especially in vascular parts, where many small vessels have to be treated; the needles and wires become inextricably confused, the tissues become puckered and displaced, and the withdrawal of the needles impossible.

The treatment of aneurism is well handled and carefully described. Dr Pirrie prefers compression to ligature, at least at first, in all cases, and speaks in warm terms of the skill and courtesy of the Dublin surgeons, with whom, in 1859, he carefully studied the question.

In his estimate of the merits of galvano-puncture, Dr Pirrie has evidently not had the advantage of studying the later researches on the subject, especially those of Dr John Duncan of Edinburgh, or his verdict would not be so discouraging. His views regarding the principles of the operation are not quite in accordance with recent teaching; for though "stratified fibrine" is better than "coagulated blood," recent cases and experiments have shown that coagulated blood is not to be despised as a starting-point for the solidification of an aneurism. In the discussion of another contested question of practice, a very curious instance of misquotation occurs, which we must correct. As a proof of the unsatisfactory results of primary amputation at the ankle-joint in cases of dislocation, Dr Pirrie (p. 452) quotes Syme as follows:—"Mr Syme stated, in the *Edinburgh Monthly Journal* for August 1844, that of thirteen operations only two of the patients recovered." What Mr Syme really did say was:—"In the Royal Infirmary, I find that of thirteen patients who had suffered compound dislocation of the ankle, and were *not* subjected to amputation, only two recovered." To the very last, Mr Syme strongly recommended amputation in most cases of compound dislocation in hospital.

The further on we go in the perusal of this work, and especially

in the more strictly practical sections, we find many wise decisions, the product evidently of a matured and well-used experience, and which are of great value for the guidance of the profession. What Dr Pirrie has worked out for himself, and stamped with his own individuality, is to our mind much more valuable than what he has tried to assimilate from the labours of others. One or two of his *dicta* we now merely quote :—

"The treatment of varicose veins is either palliative or radical; the former has now almost entirely superseded the latter, and, in the opinion of the writer, ought always to be preferred, except when the disease endangers the life of the patient."—P. 698.

"For accomplishing a radical cure of hernia, various proceedings have been adopted besides the use of a truss. Some of these were the operations of Guérin, Bonnet, Pancoast, Gerdy, Syme, Redfern, Davies [by the way, Redfern Davies is one man, not two—*Reviewer*], Wützer, and Rothmund. In my opinion they should all be abandoned, and the admirable operation of Professor Wood of London should be preferred,—an operation sound in principle, and thoroughly proved to be perfectly successful.—Pp. 704-5.

As to lithotomy, of which the Aberdeen surgeons have large experience, Dr Pirrie says :—

"The modification of the lateral operation introduced, and also successfully and skilfully practised, by the late lamented Mr Liston, is, in the opinion of the author, the simplest, the best, and the most elegant of all methods; and should, in almost every instance in which lithotomy is justifiable, be preferred."—P. 760.

His own modification of Pirogoff's operation is thus described :—

"The surgeon, standing on the left side of his patient, with the heel, in the first step of the operation, directed towards himself, and having with his left hand taken hold of the soft parts, and drawn them a little backwards, so as to secure greater breadth of flap, inserts the knife on one side in front of the malleolus, carries it down across the sole of the foot, and upwards to the corresponding point, just at the front and upper part of the other malleolus, taking care to direct the incision so as to pass opposite to the part where the posterior portion of the astragalus rests upon the calcaneum, and to use the knife energetically, so as to cut through all tissues down to the bones. By this single movement of the knife, a clearance is made for the saw, by a few movements of which the portion of calcaneum behind the astragalus is speedily cut off from the rest of the bone, the section being from below upwards and a little backwards, so that the portion remaining in the flap will be a little longer from behind forwards, below than above. The assistant having slightly changed the position of the leg, so as to make its posterior part to rest upon the table, the surgeon, by a second movement of the knife, unites the extremities of the first incision by a slightly semilunar incision, using the knife boldly, so as to cut through every tissue in front of the bones, and then, by a few slight touches below, admits of the flap being brought back, and makes a clearance for the saw. By a few movements of the saw the bones are cut through immediately above the ankle, and this extremely simple amputation is completed by little more than two movements of the knife, and two sets of movements of the saw."—Pp. 802-3.

On excision of the elbow-joint we have the following very satisfactory résumé of results :—

"I have now performed this operation one hundred times, and of that large number I have only had two deaths, both of which occurred many years ago,

one having been caused by phlegmonous erysipelas, and the other in a patient—a young man—in whose case I yielded to earnest entreaties to amputate his leg and excise his elbow-joint at the same time. In no case has amputation been required, and, as a rule, the results have been most gratifying. I have not amputated an arm for disease of the elbow-joint for many years. I never hear of such an amputation without regret and astonishment, and I firmly believe it very seldom can be necessary.”—P. 809.

The last quotation is a short one,—“Against internal division of stricture I have always entertained a strong prejudice.”—P. 860.

The diagrams which illustrate the work are, in a large proportion of cases, the regular old favourites from Fergusson and Liston. Some we recognise as having adorned Miller's surgery; others have more lately been adopted by Gant. Would it be considered unfeeling if we said we were nearly tired of them? The correcting of the press has been badly done as far as proper names are concerned—a great many are misspelt, and even spelt in different ways within a few pages.

Modern Surgery, its Progress and Tendencies: Being the Introductory Address delivered at University College at the Opening of Session 1873-4. By JOHN ERIC ERICHSEN, Professor of Clinical Surgery in the College; Senior Surgeon to the Hospital, etc. Pamphlet, pp. 28. London: H. K. Lewis: 1873.

FROM the somewhat monotonous level of the ordinary introductory lecture—a monotony not to be wondered at, hardly to be regretted—there are generally one or two lectures every year which have an individuality of their own, either from the circumstances in which they were given, or the matter of which they treat. This lecture of Mr Erichsen's, written with his usual facility, and with more than his usual grace, seems to us to strike a key-note somewhat higher than usual on the question of what it is that the surgery of the future has to do.

The surgery of the past, he says, has had its day:—“There cannot always be fresh fields for conquest by the knife. There must be portions of the human frame that will ever remain sacred from its intrusion, at least in the surgeon's hand. That we have nearly, if not quite, reached those final limits, there can be little question. When we reflect that every large artery in the human body, up to the aorta itself, has been repeatedly ligatured,—that each of the six large articulations, and many of the smaller bones, have been resected; that the amputation of each limb, up to the shoulder and hip-joints, is a matter of ordinary surgical occurrence; that large tumours, having the most intricate anatomical connexion, have been removed from every surgical region in the body, from the base of the brain to the lowest organ of the pelvic cavity,—when we reflect, I say, on triumphs of the surgeon's art that are expressed by operations such as these, we can scarcely believe that much

remains for the daring of the boldest to devise, or the skill of the most dexterous to accomplish, in the extension of that art in the direction of the operative department of our profession, and that the surgeon in future must be content to repeat, though possibly in a modified and improved manner, those operations that have been inaugurated by the genius and perfected by the skill of his predecessors.

"It is true that there are yet regions that have rarely been successfully invaded by the scalpel, though they have been contemplated as possible seats of future surgical operations; but it has yet to be determined whether the extirpation of the kidney and of the spleen is more than a bold experiment on the power of endurance of the human frame; whether it is a surgical triumph or an operative audacity."—Pp. 11, 12.

To make success of an operation equal the skill with which it has been performed, is the task of the surgeon of the future. This introduces the vexed question of hospital results, and Mr Erichsen points out the enormous fatality that still attends the greater amputations, especially in hospital practice. The results of ovariectomy in private practice and in small hospitals are appealed to, and Keith's successes quoted as an illustration of the success of an operation which has been rendered a warrantable one only by the good results of country and private cases, after the hospital cases had nearly all proved failures.

To this field of labour (that is, to diminish mortality after operations, especially in hospitals) Mr Erichsen directed the attention of those students and practitioners who had the good fortune to listen to this very interesting and instructive address.

The Causes and Treatment of Deafness: Being a Manual of Aural Surgery, for the Use of Students and Practitioners of Medicine. With Illustrations. By JAMES KEENE, F.R.C.S.E., M.R.C.P., Assistant-Surgeon to the Central London Ophthalmic Hospital; late Surgeon to the West London Hospital, etc. London: Robert Hardwicke: 1873.

DISEASES of the ear are little understood and much neglected by the great bulk of the medical profession. We hail with satisfaction any attempt to remove this unsatisfactory state of things, and to lay down clear and sound principles of the diagnosis and treatment of aural disease.

Mr Keene, in the handy manual before us, has fulfilled a want long felt by the student and busy practitioner. At once simple, clear, and concise, and yet at the same time containing all the most valuable results of the most recent advances in aural pathology and surgery, Mr Keene's manual is the best introduction to the study

of diseases of the ear we have seen. He divides his subject into two parts. In the first part are described the different methods and apparatus employed for the purposes of diagnosis and treatment, together with the various normal and pathological conditions of the auditory apparatus; the second is devoted to the consideration of special diseases.

In both parts the author, while giving the usual principles and modes of practice adopted by others, asserts his own individuality in many original and valuable suggestions. We think his method of recording the hearing distance particularly worthy of quotation: "Having ascertained, on persons of normal hearing, the distance at which the tick can be heard, this constitutes a standard by which we can judge the amount of hearing in disease. Watches, however, differ so much in tone, and consequently in the distance at which they can be heard, that to describe a watch as being heard at so many inches from the ear, really means nothing except as regards the particular watch employed. I would therefore recommend keeping record in a manner very similar to that used in testing vision. Thus, the watch may be represented by the letter W, and supposing the distance at which it can be heard by the healthy ear be five feet, i.e., sixty inches, $W\frac{1}{5}$ would express the normal hearing distance corresponding with the emmetropic condition of the eye. If, in the case under observation, the watch can only be heard at one foot, i.e., twelve inches, $\frac{1}{5}$ would represent the condition of hearing. In each case the denominator of the fraction indicates the number of inches at which the watch can be heard in health; the numerator, the actual distance in the case under observation. When the distance is less than an inch, the addition of a cypher to the denominator would express the quantity in tenths; thus, $\frac{1}{50}$ would represent half an inch, and so forth. Contact may be expressed by $\frac{0}{5}$; pressure, $\frac{+}{5}$; and if not audible at all, $\frac{-}{5}$. This method possesses the advantage of showing at a glance the distance at which the watch should be heard, as well as that at which it is heard." His directions for the use of the tuning-fork in order to determine the situation of the disease, whether in the conducting media or in the nerve structure itself, are simple, and will enable a student at once to appreciate the delicate diagnosis which may be made in cases of deafness. Many valuable suggestions and improvements in the aural speculum and its use, and in the methods of illumination, deserve to be commended. In describing the situation of disease on the membrana tympani, the method of division into quadrants is adopted, and Gruber's beautiful figures are made use of for the purpose. The appearance of diseases of the membrane are well described, and their diagnostic value clearly stated. A useful hint is given that we should always employ the Valsalvian method of inflating the tympanum during the examination of the membrane by the use of the speculum. In this way many important conditions may be discovered, and much light thrown upon

the cause of the aural disease. His method of passing the eustachian catheter seems to us to fall short of the method so plainly and clearly described by Gruber in his work "*Lehrbuch der Ohrenheilkunde*," pp. 204 to 208. Allan, in his work on "*Aural Catarrh and Curable Deafness*," page 86, gives the shortest, at the same time the simplest, description of the operation, but it can only be useful to one who has had much experience in the operation. Mr Keene justly insists on an accurate examination of the throat and fauces at the same time that the ear itself is the subject of examination. "The great frequency with which the middle ear is affected by extension of disease from the throat, renders it imperative that the part be carefully examined." Very properly he strongly recommends and describes the method of using the rhinoscope in the examination of the faucial end of the eustachian tube.

In his treatment of aural disease, our author is as usual clear and sound. His directions for using the aural syringe are very good. "Syringing the external meatus should never be performed unless we have previously ascertained by the use of the speculum, that it contains something to be removed." He describes a method we have found very beneficial in our own hands, in cases where considerable destruction of the *membrana tympani* exists, with foetid purulent discharge from the internal ear, chiefly occurring in weak constitutions, viz., the injection of an alkaline tepid solution, either by the eustachian catheter, or by forcing the fluid from the external meatus through the tympanum, eustachian tube, and nostril. He states—"In this manner we shall remove masses of tenacious dark-coloured mucus, of large size, which have probably been lodged in the mastoid cells." We have seen such masses removed from the nostril, adhering to the eustachian catheter, and think they have their origin in the nostril. He has no great faith in blisters, rightly preferring the injection of medicated vapours and solutions by the catheter. In cases of acute pain, he advocates the use of local abstraction of blood. "When we wish to take blood in affections of the meatus or tympanic membrane, the proper place to apply our leeches will be immediately in front of the tragus; for the middle ear, the best place is below the lobule in the fossa which separates the lower jaw from the mastoid process; when the internal ear, the membrane of the brain, or the bone substance, are affected in connexion with ear disease, we ought to draw blood from the surface of the mastoid process behind the ear." He recommends myringotomy only as a means of evacuating purulent or other matter from the tympanic cavity. Notwithstanding the account given of seventeen cases of inflammation of the tympanum following the use of the nasal douche by Dr St John Roosa of New York, our author is in favour of its use. We have seen its careless application do harm.

Part II. is devoted to special diseases. Under diseases of the

external ear, he includes diseases of the auricle, meatus auditorius externus, and the external surface of the membrana tympani. We wonder how many minutes there are in the "short time" in which our author considers the auricle may be successfully reunited to the trunk after its complete separation. Tumour of the lobule is not entirely confined to the coloured population of the West Indies. We have met with cases in Edinburgh. The non-coloured subjects of this deformity here, blame the colouring matter upon the earrings, not to their being of brass, as Dr Roosa of New York gives as the cause. We have made sections of these tumours, and found a black deposit in the fibrous layers immediately surrounding the perforation. We are almost tempted to give in full his chapter on "Foreign Bodies" in the external meatus, but would advise all medical practitioners to obtain the work for themselves, as by reading this chapter many will be amazed to find how easily a foreign body can be removed from the external meatus, provided there is one there.

For the thorough eradication of polypi and granulations, Mr Keene speaks highly of chloro-acetic acid as a means of accomplishing a duty not at all agreeable to patient or doctor under the formerly adopted method of treatment.

In the middle ear, he includes the tympanum proper, with its prolongation in the eustachian tube and mastoid cells, as well as the ossicles, ligaments, membranes, and muscles which it contains. "The affections of the middle ear, from which all others may be considered to originate, are catarrh and inflammation, each of which may be acute or chronic. Catarrh is simply hyper-secretion, resulting from an augmented supply of blood to the lining membrane of the tympanum. This may terminate in resolution by subsidence of the hyperæmia, the parts resuming their normal condition; or in mucous accumulation from the secreted products being retained. If exudation take place from the bloodvessels, the case becomes one of inflammation. This also may terminate in resolution by absorption of the inflammatory products and return of the tissues to a healthy state. It may proceed to suppuration by extra proliferation of cells, which become pus, or it may result in plastic deposition of the exuded products, remain in the tissues, and become organized. Suppuration and plastic depositions, which are consequences of inflammation, may themselves terminate in further sequelæ. Thus, the purulent formations of suppuration may escape by the eustachian tube; they may cause perforation of the membrana tympani, or the destructive action may extend to the periosteum and bones, giving rise to caries or necrosis, which may even extend to the cranial cavity and cause death. When the exudation is plastic, this may produce adhesion among the parts within the tympanum, or thickening and hypertrophy of the lining membrane, which may become the seat of osseous or calcareous deposits. Thus, perforation of the membrana tympani and caries or necrosis of the

bones may be considered as consequences of suppuration; sclerosis, ankylosis, calcareous degeneration, and polypus, as results of plastic deposition. . . . Polypus, moreover, is much more frequently preceded by suppuration than by that form of inflammation the products of which are plastic. In such cases, however, I believe that plastic action becomes superadded to the suppurative, and that the two forms coexist."

In the chapter on Aural Catarrh, the symptoms and physical signs are most accurately described. A simple attack of acute catarrh spreading from the throat, in an otherwise healthy person, never gives rise to deafness, further than what is usually felt in the simple nasal catarrh so frequent in our northern climate. The form usually described corresponds to Mr Keene's chronic catarrh. We quote from his chapter on the Morbid Anatomy of Chronic Catarrh. "The disease I wish to describe is a chronic one, affecting the secreting structure of the middle ear, the activity of which is thereby excited, but without the occurrence of interstitial exudation, which only takes place in inflammation. The same distinction exists between catarrh and inflammation in the middle ear as between catarrh of the pharynx and pharyngitis, and in most cases it will be found that catarrh of the nose and pharynx will cause the same affection in the tympanum, whilst pharyngitis will generally be accompanied by inflammation of the lining membrane of the middle ear. Chronic catarrh of the tympanum is not unfrequently met with in adults; it is, however, much more common among children, particularly in those of a strumous diathesis, in whom the general soft and spongy condition of mucous membrane may be presumed to extend through the eustachian tube to the middle ear. The tonsils and other glandular structures of the nasal and pharyngeal surfaces are generally enlarged, and their secretion abnormally augmented."

Acute inflammation of the tympanum most frequently extends by the eustachian tube from pharyngitis occurring in the course of the acute exanthemata. The occurrence of this dreadful malady in early childhood may be arrested by the application of leeches below and in front of the meatus. The difficulty lies in the diagnosis of affection of the ear in those cases of acute exanthematous diseases where the practitioner's attention is taken up by the severity of the other symptoms. Mr Keene advises the application of two or four leeches in children, where pain in the head is detected by the little sufferer's putting the hand to the side of the head. Our author tells us how we may diagnose chronic catarrh from chronic inflammation, by the use of the tuning-fork. In catarrh, the tuning-fork, placed on the vertex, or better, on the nasal bones, is better heard than it would be in health. In inflammation, the reverse holds good. Then, by inflating the tympanum after Politzer's method, more benefit is experienced in catarrh than in inflammation. In a case of inflammation, by the use of the speculum, we will see the

thickened fibres of the external fibrous layer of the membrana tympani. These will be better noticed after inflation of the tympanum. Siegle's speculum will assist us in noticing the existence of adhesions of the membrane to the walls of the tympanic cavity. We are not sure how far our author's ideas regarding the formation of plastic deposits will stand the test of rigid pathological investigation; still, his remarks and views on the subject are worthy of the most careful consideration.

We are sure the work from which we have given so many extracts will fully bear out all the encomium we have given it at the beginning of our notice.

Eléments de Chirurgie; Clinique, comprenant le Diagnostic Chirurgical, les Operations en Général, les Méthodes Opératoires, l'Hygiène, le Traitement des Blessés et des Opérés. Par J. C. FELIX GUYON, Chirurgien de l'Hôpital Necker. Paris: 1873. 1 vol. 8vo, de xxxviii-672 pages, avec 63 figures intercalées dans le texte.

Elements of Clinical Surgery; comprehending Surgical Diagnosis, Operations in General, Methods of Operating, Hygiene, the after Treatment of Wounds and Operations. By J. C. FELIX GUYON, etc.

THIS large and important work is not exactly one on clinical surgery in the English sense of the word; there is not a single clinical case in the book. Nor is it necessarily the extension or result of any bedside study whatever. It answers rather to the works in English literature which treat of minor surgery, splints and bandages, microscopes, test-tubes, and other surgical or medical appliances; and it also contains a large section devoted to an account of various methods of making a diagnosis, risks of error, and methods of avoiding such risks. A large amount of information is stored up in its pages, given in rather a wordy, tedious manner, but still useful and accessible by means of an excellent index. M. Guyon, in his preface, counsels the student to enter the hospital at once, and to be diligent there; and hopes that this work will enable him at once to understand the disease and treat the patient, though in it no information is given as to the diseases or injuries themselves.

A historical introduction exhibits considerable research, and briefly notices the chief names in surgery, from the earliest times to the end of the eighteenth century. The names of British surgeons are not forgotten, though, naturally enough, the great names of French surgery bulk largest in the list, but the appreciation of many of them is cordial and well considered; thus—

"Benjamin Bell represents in England Heister, or our own Boyer. His System of Surgery is a classic work, consisting of a

series of monographs which comprehend all surgery, and in which we find the vast experience of a great master."

"Percival Pott, in his numerous papers on wounds of the head, on hydrocele, and specially on amputation, and on vertebral caries, to which disease he has given his name, and also on hernia, has deserved to be placed on a level with Petit."—P. xxxvii.

The first large section or chapter is devoted to the means of diagnosis of any given case, under the head of history of case—state on admission—special studies of certain symptoms, as fluctuation, oedema, pain—manner and instruments of exploring, including ophthalmoscopes, laryngoscopes, etc., percussion, auscultation, tests, etc. Then the method of arriving at a diagnosis and establishing it is given, and lastly the causes of difficulties, uncertainties, and errors of diagnosis are detailed. In this chapter the student will find a large amount of information well systematized. Much of it is equally suited for an introduction to clinical medicine, but still not the less valuable. In some points we totally differ from the author: "that the dorsal position is the best for the introduction of a speculum" (p. 30), and that "in passing the female catheter it is always best, in every point of view, to expose the patient" (p. 71), are not safe doctrines to teach students in this country.

Anæsthesia and anæsthetics are pretty fully described historically, chemically, and physiologically. As to the choice of an agent, the dangers of ether and chloroform are fairly discussed and balanced, and the rule given with great emphasis that *no patient should ever be anæsthetized in a sitting posture*. The surgeon is advised always to have in readiness ammonia, vinegar, artery forceps, a catheter to inflate the lungs, the instruments requisite for tracheotomy, and a galvanic battery, to be used as recommended by MM. Legros and Onimus. It is also advised never to administer an anæsthetic without the presence of a competent assistant. A second assistant entirely occupied by watching the pulse is, in our opinion, an unnecessary luxury, even a dangerous one, if the pulse is trusted to as a proof of the safety of the agent, apart from the far more important respiration.

We next have an account of the general principles of operations, indications for and against operation, preparation of the patient, preparation of the surgeon. Then a very well arranged, but terribly long-winded, account of minor surgery, including aspiration and skin-grafting. Venesection at the bend of the arm has eighteen pages devoted to it, while tracheotomy, which curiously enough comes in under minor surgery, has only eight pages. Dr Alexander Wood gets the credit he deserves for the introduction of the syringe which bears his name, and of the operation of hypodermic injection (p. 363).

The third chapter, on the treatment of wounds and the after-treatment of operation cases, contains a long account of hospital hygiene, a very favourable estimate of alcohol as a dressing for

wounds, and a most exhaustive description of the various modes of dressing wounds. Through this the student may wade without much indication as to which mode is the best, or whether any may be considered useless or behind the age. A translation by M. Terrier of Holmes's account of carbolic acid dressings, as they existed in 1871, is quoted without comment of any kind. It also contains an account of all the methods which are or have been in use of making bandages and splints, only from the mechanical or constructive point of view, not giving any information as to the kind of fracture or joint-disease for which they are needed. This concludes the work; which we leave with the impression that it is a laborious, conscientious description of part of the surgeon's armamentarium, but not what we call a work on the Elements of Clinical Surgery.

A New and Easy Method for the Subcutaneous Application of Morphia, without the Pain, Risk, and Expense of the Hypodermic Syringe. By JOHN M. CROMBIE, M.A., M.D., Resident Medical Officer, Cancer Hospital, Brompton. London: Mackey and Co. Pamphlet, 8vo. Pp. 16.

THE author has a high estimate of the value of the local use of morphia, and believes that many medical men are prevented from using such means by the expense of the syringe, its frequently needing repair, and that patients object to the pain it causes. His method of avoiding these misfortunes, is to use small threads coated with the requisite quantity of morphia, which are inserted by means of fine needles into a fold of skin, pinched up between the finger and thumb. Theoretically, the plan is ingenious and excellent. The practical difficulty that we would fear is, the risk of the morphia either not getting into the wound, by being rubbed off the thread, or being so well incorporated with the thread as not to be dissolved out of it during the time it is retained, which is to be from three to four minutes. The suppositories, as Dr Crombie calls the threads, vary in strength from $\frac{1}{8}$, $\frac{1}{4}$, $\frac{1}{2}$, to 1 gr. of morphia. The larger ones are not often necessary. One needle will do almost any number of times; the threads are to be used new every time.

The Sanitarian: A Monthly Journal. A. N. BELL, M.D., Editor. A. S. Barnes and Co., New York and Chicago. Nos. I.-V. April-August 1873.

WE have received the first five numbers of this new periodical, devoted to the subject of sanitary science—the laws of physiology and pathology as the basis of health—state medicine—sanitary

architecture—light, space, warming, and ventilation,—all that will aid in the protection of human life.

This magazine deserves success. It is devoted to a most interesting and important subject. It seems to be begun with much energy and in good tone; it is most liberally "*published*"—that is, paper and type are all that can be desired; and the illustrations, maps, plans, etc., are on the most luxurious scale.

In No. I. we marvel to find the sanitary progress of Britain spoken of in the most complimentary way. This is encouraging, though we have still much to learn. The Editor contributes an instructive paper on the New York Quarantine Establishment, with Sketches of the Buildings on the Islands of West Bank and Hoffmann.

In No. II. there is an excellent paper on the Sanitary Influence of Light, by Dr Hammond; another on Free Parks and Camping Grounds for Children, by Dr Toner. We, in this crowded old world, can only with immense expense *make* free spaces in or near towns; cannot the newer American towns wisely *keep* plenty of free space for lungs to the town?

In Nos. III. and IV. the Nursing and Artificial Feeding of Children is discussed by Dr Jerome Walker with much practical sagacity; and there are other valuable papers on Defective Drainage and Sanitary Architecture.

No. V. contains a most excellent article (illustrated by an admirable chromolithograph of a section of the house), entitled "School Poisoning in New York." The section shows the various classes at work, and a gorgeous scheme of ventilation by a furnace also supposed to be at work, only that the place for the ingress of fresh air had been made into a poultry-house (*foul* air indeed), and the aperture of exit was a dovecot!! There are other excellent practical papers—one describing Colonel Storer's method of treating the animal refuse of great cities; another pointing out the dangers of damp cellars, of defective drainage, etc.

We wish this new magazine every success, and believe it may be of immense service in America, in arousing not only the profession but the public to consider how much can be done by sanitary and hygienic means to prolong life and diminish disease.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LIII.—MEETING I.

Wednesday, 5th November 1873.—Dr HANDYSIDE, *President*, in the Chair.

I. The *President* presented to the Society a copy of a paper on LEPROSY IN SYRIA, by Dr Wortabet.

II. *Dr Argyll Robertson* exhibited a preparation of GLIOMA OF THE EYEBALL. The patient was a child eighteen months old. A few weeks after birth, a yellow reflection from the pupil was observed by the mother; but no special attention was paid to the eye till about two months before Dr Robertson was consulted, when a small growth or projection was noticed close to the inferior margin of the cornea, and the eye became inflamed. The projection increased rapidly in size, measuring about $\frac{1}{2}$ inch in diameter, and being about $\frac{1}{2}$ inch in height. The tension of the eyeball was very much increased, the pupil was occluded with lymph, and the iris pushed forwards so as almost to touch the inner surface of the cornea. The malignant nature of the disease being apparent, immediate enucleation was recommended as the only feasible treatment, and, being acceded to by the parents, had recourse to. As, after removal of the eyeball, the portion of nerve remaining appeared diseased, it was also excised as far as the apex of the orbit. Even at this point the nerve did not appear quite healthy, and the prognosis was consequently unfavourable. About two months ago (two months after operation), Dr Robertson again saw the child, and found a small tumour occupying the orbit—evidently a return of the disease. The tumour presented all the characters of a glioma. A special feature of interest in the structure of the tumour was the presence of numerous calcareous particles towards the centre of the growth. This was to such a degree as to necessitate the use of considerable force with a powerful knife to effect a section. Calcareous degeneration of a malignant growth in so young a subject is deserving of record.

III. *Dr Joseph Bell* exhibited a specimen of EXOSTOSIS OF THE PROXIMAL PHALANX OF THE GREAT TOE. The patient, a girl of 20, had four years previously been kicked by a horse on the foot; this tumour had followed the injury, and had recently grown rapidly. The tumour, which was as large as a small apple, grew from the whole outer edge of the bone, involving and overlapping both its articular ends, displacing, but not involving, the second toe. It interfered

with walking. Unwilling to sacrifice the great toe, Dr Bell exposed the tumour freely, and then split the phalanx longitudinally, removing the whole outer side of it, and the outer half of both its joints. He treated the case with antiseptic precautions, and the patient has now, three weeks after the operation, full movement of both the joints.

IV. *Dr Stephenson* then read a paper on PNEUMONIA, as observed in children—which will appear in the January number of the Journal. It was fully illustrated by clinical charts of pulse and temperature, from which the author endeavoured to produce a clinical history of the acute primary form of pneumonia. He detailed several interesting cases at greater length, and finished by some observations on the subject of *tension* as an element in the consideration of the pulse, and the mutual relations of pulse and tension to temperature.

The *President* complimented the author on his excellent paper, referring especially to the value of his observations on *tension*, also on treatment, and its value in cutting short or modifying the disease, and on the careful charts, so valuable as a foundation for future results.

Dr Matthews Duncan thought the Society were much indebted to Dr Stephenson for the paper. He had been specially struck by the charts of acute primary pneumonia, illustrating the sudden rise of temperature, the sort of tableland of high temperature lasting for five to seven days, and then the sudden fall. He thought that the regularity of these symptoms warranted Dr Stephenson in isolating that group of cases from the rest under a special head. The other cases did not seem to him to be so distinct.

Dr G. W. Balfour, after alluding to the importance of the paper, and the difficulty of criticising it on such short notice, said that, for his part, he was inclined to agree with Wunderlich, who holds that in pneumonia temperature is not of much importance. He recognised three types of pneumonia in adults, excluding catarrhal pneumonia and broncho-pneumonia. 1. One in which the temperature rose with suddenness and fell equally suddenly. In this embolic or oedematous form no treatment was needed. 2. One in which a sudden rise was followed by a high average temperature for five or seven days, and then a sudden fall. This he believed can be cut short by treatment. 3. One in which a gradual rise, extending over three days, was followed by three or four days of high temperature, and an equally gradual fall. In this he believed treatment was also useful. He was interested in the question of tension, and hoped soon to bring before the Society cases of pneumonia in the adult, as illustrating the results of treatment.

Dr Hammond asked Dr Stephenson if the chlorides were diminished in the urine in these cases he mentioned, in which the physical signs were almost negative. Dr Stephenson said they were always diminished.

Dr James Carmichael then mentioned that in three cases of pneumonia in children, he had lately observed a nervous symptom, namely, a limited hyperæsthesia over the skin corresponding to the portion of lung affected. He also noticed the frequency of cases of pneumonia of the upper lobe especially being overlooked in practice. He thought it would be interesting to trace the future history of cases of apical pneumonia, to see how far the disease had originated a predisposition to phthisis in after-life.

In reply, *Dr Stephenson* had not noticed the third set of cases alluded to by *Dr G. W. Balfour*. He believed that variations in the clinical history were often caused by distracting influences, of which treatment was a most important part, and alluded to the observation of *Barthez*, that recovery is slower in cases where active treatment has been adopted.

V. Dr Miller then read a paper entitled REMARKS ON THE EMPLOYMENT OF BULBOUS BOUGIES, which appears at p. 515 of this Journal.

Professor Spence does not place nearly so much dependence on the *form* of the instrument as on the *manner* in which it is used. In very small instruments, however, certain forms are more useful than others. So long ago as 1843, he had got made for himself, to use in a bad case of stricture, a probe-pointed bougie. This was from observing in post-mortem examinations of cases of stricture how easily the common surgical probe was passed through even the tightest stricture. The ordinary No. 1 bougie has too sharp a point, and itself is too flexible; but the probe point and conical stem of his probe-pointed bougie give it both safety and strength. He saw no use whatever in a bulbous point in instruments of large size. *Dr Miller* had not said much in favour of soft bougies, and so far he agreed with him. In some cases of prostatic retention, he recommends the use of the old-fashioned gum-elastic catheter, the point of which has no risk of bending back or breaking off. In difficult cases the rigid instrument is best in the surgeon's hands, while a soft one perhaps may be safer in the patient's hands in easy cases.

Dr Joseph Bell was not a great admirer of soft instruments, except in easy cases of retention. He called the Society's attention to a lecture by *M. Demarquay*, of which an abstract appears in the November number of this Journal (p. 457), on the dangers of these soft catheters when badly constructed,—the risks of being injured by putrid urine, breaking in bladder, or injuring urethra. For his own part, he used these bulbous catheters more as means of diagnosing the length and position of strictures than of treating them.

Dr James Carmichael related a case in which, in attempting to relieve himself of retention from enlarged prostate, a patient had drawn a large quantity of blood, and failed to reach the bladder, having broken and twisted a soft catheter, though it was quite new.

Dr Chiene asked Professor Spence as to whether it would not be easier to pass a bulbous instrument through a stricture than a cylindrical one of the same thickness?

Mr Spence explained how the conical shape of the stem of the bulbous instrument did away with any advantage gained by the bulbous shape of its end.

Dr Miller agreed with Professor Spence and *Dr Bell* in much they had said about the uselessness of the bulbous bougies in the treatment of most forms of stricture, and also the danger of leaving them too long in patient's hands, but said he advised his patients to have several instruments in their possession, and to throw them away on the first symptom of any chafing or injury from the urine.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXII.—MEETING XVI.

9th July 1873.—*Dr Thomson*, *President*, in the Chair.

A case of INTRAUTERINE POLYPUS, by *Dr Slavjansky*, St Petersburg, and *Dr Matthews Duncan* (which appeared in this Journal for August, p. 97).

Dr Milne, after some complimentary observations on the value and interest of the paper, observed that he had seen several cases in which retroversion brought on abortion, but in these he found the uterus was adherent. He was of opinion that pregnancy was one of the best cures for retroversion, but also considered that retroversion was more likely to produce abortion than anteversion. In the second case mentioned by *Dr Slavjansky*, he thought that he would have waited longer before bringing on abortion.

Dr Keiller remarked that he had nothing very special to say on the subject of the paper, which, however, he considered a most instructive one. This endometritis, as described by *Dr Slavjansky* so fully, is a very common cause of abortion without there being any displacement of the uterus.

Professor Simpson observed, we often have cases of abortion set down as due to flexions; but then *Dr Slavjansky* goes a step further, and says it is the endometritis which is the cause; but here he would have gynaecology at issue with him. He says, there is first a flexion, and, as a result, endometritis after conception has taken place. I am somewhat inclined to agree with him, and with those gynaecologists who take his view. The paper he thought a most interesting one, and he questioned, if in our own language we have any paper which so well and thoroughly explains the pathological changes, uterine and ovuline, in such cases.

Dr Slavjansky, in reply, thanked the Society for its reception of his paper, and made some further observations on the relations between flexions and endometritis.

Part Fourth.

PERISCOPE.

DERMATOLOGY.—SCLERODERMA—Dr Van Harlingen (*American Journal of Syphilis and Dermatology*, October 1873) thus defines this skin affection: "Scleroderma is an affection of the skin, characterized by diffused symmetrical hardening, and generally accompanied by more or less pigmentation. Beginning in some particular locality, without febrile disturbance, swelling, or œdema, the induration spreads with greater or less rapidity over a considerable extent of surface. The affected parts fade gradually into those which remain healthy without any distinct line of demarcation, and they are on a level with the general surface, neither raised nor depressed, and contain no tubercles or other elevations. The temperature of the affected surface is either normal or slightly depressed. The general sensibility, as well as the functions of the glands, are unaffected, save in the advanced stages of the most severe cases. It runs a chronic course uninfluenced, or nearly so, by therapeutical applications, and has no tendency to a fatal termination. Pathologically, the disease consists essentially in a great increase in the fibrous elements of the corium and papillary layer, decrease of fat in the subcutaneous connective tissue, and deposit of pigment in the lower layers of the epidermis or in the rete." In an analysis of twenty-eight cases, the author found that twenty of them occurred in females. Rheumatism seems to be a frequent concomitant of the disease.

BROMIDE OF CALCIUM IN THE TREATMENT OF SYPHILITIC NEURALGIA.—Some cases showing the benefit derived from the administration of bromide of calcium in this form of neuralgia have been cited by Dr W. A. Hammond (*American Journal of Syphilis*, October 1873). This drug is given in fifteen or twenty grain doses, and was successful in curing cases where all the usual remedies had been tried in vain. Dr Hammond says that it failed in three cases not dissimilar to those in which it succeeded. It certainly is, he says, so far as his experience extends, much more efficacious than any other of the bromides, most of which he has tried in syphilitic neuralgia with but indifferent results.

METALLIC CATHETER BROKEN IN THE URETHRA.—Dr Cazaux of Oloron reports the following case:—A man of 66, unable to make water, had a metallic catheter introduced into his bladder, and left there for a fortnight, after which another was inserted. This broke after a week's stay in the bladder, leaving a considerable

portion in the membranous portion of the urethra. In this condition the patient remained without seeking advice for three weeks more, when Dr Cazaux was called in on account of retention of urine. He was able to feel the instrument in the urethra, and before incising it, tried the following simple plan. Holding the broken catheter firmly between his finger and thumb, he inserted a flexible bougie 4 millimetres in diameter into the urethra, and after some attempts succeeded in passing it into the broken catheter. He then forced it in as far as possible, still holding the other firmly, and by that means succeeded in getting a sufficiently firm hold of the broken piece as to be able to withdraw it.—*Gazette Hebdomadaire*, No. 47, 1873.

HYPODERMIC ATROPIA IN CHOLERA.—The following, from Dr R. Saunders, of Paducah, Ky., a practitioner of age and very great experience, though not written for publication, will, we are sure, make very interesting reading just now to physicians who are dealing with or anticipating the approach of cholera:—"In the recent outbreak of cholera in Paducah, I treated a number of cases by sulphate of atropia hypodermically—one-fiftieth to one-thirtieth of a grain in water—with the happiest results. The more distressing symptoms—vomiting, purging, and cramps—were relieved almost at once, followed by refreshing sleep, continuing in some cases for several hours. I found these effects, however, to follow only when the atropia was used in sufficient quantities to produce the specific scarlatinal rash, dry throat, and dilatation of the pupils. In some cases the relief afforded was astonishing; the skin grew warm, the pulse rose, the surface, previously clammy and shrivelled, assumed its natural condition, and in some instances the patient slept soundly for ten or twelve hours, the bowels remaining undisturbed during the entire time. Of course you will not understand me as advocating the exhibition of the atropia to the exclusion of all other means, especially the use of calomel, to which I attach much importance. In the first case in which I gave the atropia I combined it with morphia (one-fortieth grain of atrop. sulph. to one-sixth grain morph. sulph.), and I think the combination is better perhaps than the atropia alone.—*American Practitioner*, July 1873.

THE SACCHARIFYING INFLUENCE OF INFANTS' SALIVA (*Schiffer*).—It has been hitherto generally believed that the secretion of the mouth of sucking infants does not possess the power of converting starch into sugar, as does that of older persons. The author of this paper gives certain observations which show this to be incorrect. He placed little bags of tulle containing starch in the mouths of new-born infants, and of sucklings at various ages. In every case he found that on applying Trömmmer's test to the contents the copper was reduced, showing the presence of sugar.—*Reichart and Du Reynaud*. Archiv, p. 4, 1873.

ACCIDENTAL POISONING BY CARBOLIC ACID.—The great number of deaths reported from accidental poisoning with carbolic acid in this country during the last year or two has attracted the attention of French surgeons, and in *Mouvement Medical* for 11th Oct. 1873, the reason of it is discussed, and a simple suggestion made for the prevention of similar accidents. The reason is found in the very concentrated form in which in this country carbolic acid is used for wounds, as compared with the weaker solutions used in France, and the suggestion is, that the strong solutions should be coloured by logwood. This would prevent the occurrence of cases of poisoning in which the patient had taken the acid instead of water; whether the logwood would interfere with its antiseptic properties, by introducing germs, is another question.

SOME REFORMS IN SURGICAL STATISTICS.—M. Verneuil takes the opportunity of reporting nineteen cases of amputation at the shoulder-joint, to analyze and criticise, with a view to its improvement, the usual method of reporting surgical statistics. The ordinary modes of reporting do not answer the questions which are really important as regards the issue of the case. Thus, it is not enough to take the results of amputations under the two heads of recoveries and deaths. Thus, case 1, a young girl of 14, who had an osteosarcoma of the head of the humerus, for which the limb was amputated, lived for three months, and died of renewal of the disease. She *died cured*.

It would be well to recognise two possible results of operation—the success or failure of the operation as such, and the success or failure of the operation as a mode of cure. In the operation for cataract, for example, the union of the flap or a successful operation does not necessarily imply a perfect cure. In fact, to determine a success, in a surgical point of view, it is necessary to note how and to what amount the operation had fulfilled the task imposed on it. Thus, successful operations may be either complete cures or partial ones; the partial ones being of two kinds—1. When a new lesion carries off the patient after the first had entirely disappeared; 2. When the original disease (such as cancer) cuts off the patient, who had completely recovered from the first operation. The column of unsuccessful cases may, in its turn, form three groups—1. Failure of the operation; 2. Notwithstanding the operation; 3. Independently of the operation.

The division of amputations into pathological and traumatic by Lawrie in 1841, and Malgaigne in 1842, is hopelessly deficient, for the least reflection will make it evident that there may be, in amputations only, at least the following varieties:—1. Accident, simple and uncomplicated; 2. Where more than one injury is present; 3. Injury, complicated with some local disease, either preceding or following the accident; 4. Injury, complicated with some general disease, either prior to or succeeding the accident, or inter-

current; 5. Disease of a part only; 6. Disease, complicated by an injury of the same part; 7. Disease, complicated either by some other local disease or some general disease, prior to or succeeding the first one.

On such principles, M. Verneuil classifies his cases of amputations under the three following heads:—1. Simple local lesion, either wound or disease—7 cases, 5 from injury, 2 from disease; result of operation, successful, 100 per cent. 2. Multiple successive lesions—3 cases, all died; mortality, cent. per cent.; result as to operation, unknown; result as to recovery, deplorable. 3. Local lesions, accompanied by general disease—9 cases, 2 recoveries, 7 deaths.

M. Verneuil then discusses at length the different amounts of information arrived at by his method and that of the old method respectively.

[Though we fear his method is complicated and difficult, yet it is a step in advance of the misleading and deficient results of a less fully classified method of stating statistical results.—*Tr.*]—From Review by Dr NEPVEU, in *Gazette Médicale de Paris*, 1873, No. 41.

ACUTE PERIOSTITIS AND SUBSEQUENT REMOVAL OF THE WHOLE CLAVICLE.—The rarity of operations for removal of the whole clavicle makes it worth while briefly to report the following case:—A girl of 18, delicate, had a severe pain over the right clavicle, for which she was advised to rub it with opodeldoc. Swelling and redness followed, for which a blister was ordered. Shivering followed, and a large abscess was formed, which was opened on the eleventh day of the disease. The clavicle was found bare, and so far movable that pressure upon it made pus escape from the wound. A secondary abscess formed in the thigh, and the patient's general health began to give way. On the seventeenth day of the disease, M. Leon le Fort put the patient under chloroform, and with great ease, and with the loss of hardly a drop of blood, removed the whole clavicle except the sternal epiphysis. It was held only by some fibres of the coraco-clavicular ligaments, which were easily divided by the scissors. The bone was smooth, and white as ivory. M. le Fort has only been able to discover sixteen similar cases in the records of surgery—by Pelletan and Mazzoni, for necrosis; by Asson, for syphilitic necrosis, with spontaneous fracture; by Roux, Meyer, Wurtzer, Biagini, Kunst, and Wedderburn, for chronic necrosis. Warren and Esmarch have done it for osteosarcomatous tumours, Chaumet for a disease which he calls fungous medullary exostosis. Another is reported, with no details, in the *Froriep Magazin*. Moreau's one was only the removal of a sequestrum, and Vinson's resembled the author's. [This list is certainly imperfect, Mott's famous case being omitted.—*Tr.*]—*Gazette des Hôpitaux*, No. 95, 1873.

SURGICAL TREATMENT OF OZENA.—M. Rouge, Surgeon of the Lausanne Hospital, has recently described a surgical method of treating ozena. The reputation of the author, the simplicity of the plan proposed, and the amount of success he has obtained (twelve in thirteen cases), give much encouragement to attempt the cure of a disease hitherto considered in many cases as incurable.

M. Rouge starts with the theory that, in all the cases, there is present some lesion of the nasal skeleton; without this the peculiar odour is not present, even in cases of ulceration or suppuration of the nasal mucous membrane. All the cases which have consulted M. Rouge have been operated on, except one, who was afraid to stand the operation; in all a bony lesion was found, which being removed, the fœtor also was at once removed. The operation is the following:—The patient is laid on his back, and put under chloroform; the head is then turned to the right side to allow the blood easily to escape. The upper lip is then pulled forwards in its centre, and put on the stretch. The mucous membrane is then to be divided in the fold where the lip leaves the gum, from the first bicuspid, on the one side, to the same on the other; the centre of the incision corresponds to the frænum of the lip, which is divided to its base. Next, the septum narium is cut through its cartilaginous base, from the anterior nasal spine. This simple section enables the operator, in most cases, to introduce his finger into the nasal fossa on each side, and, after the blood is cleansed, to examine them thoroughly. If it is not sufficient, two cuts with the scissors suffice to divide the nasal cartilages from their maxillary attachment, and thus widely to expose the nasal cavities. If the bony septum is in the operator's way, it may be necessary to divide it with the scissors, and it can be pushed first to one side, and then to the other, and thus render exploration easy. It does not seem often to be necessary to do this. Now comes the difficulty. It is necessary to explore with method, and with the greatest care, every part of the cavity of the nose, to remove all the dead bone. The necrosed portions may be hidden in the hollows of the turbinated bones, or in the vomer or ethmoid, and may be concealed and kept in position by thickened and diseased mucous membrane. Once the diseased bone is discovered, if it is necrosed it must simply be removed entire; if, on the other hand, it is a piece of carious bone with fungous projections, these projections must be removed, the diseased bone must be scraped, and then the diseased mucous membrane in the neighbourhood carefully pencilled with nitrate of silver. Then, after a thorough washing, the nose must be carefully replaced, the nasal septum being fitted exactly to its position on the anterior nasal spine. There are, as a rule, no marked symptoms, either local or general—possibly a little swelling of the nose, a little pain, even a headache; but, as a rule, by the third or fourth day, the patient is quite well. Unfortunately, in one case out of the thirteen, an attack of meningitis occurred, and proved fatal. This

was the only unsuccessful case. Most of the patients had suffered for seven or eight years,—one had had the disease for twenty-seven years. All were at once cured.

This is a most encouraging amount of success achieved over a most distressing and generally almost incurable malady.—*Gazette Médicale de Paris*, No. 42, Oct. 1873.

RESECTION OF COCCYX TO FACILITATE THE FORMATION OF AN ANUS IN THE PERINEUM IN CASES OF IMPERFORATE RECTUM.—The following are the conclusions arrived at in a long and interesting paper by M. Verneuil on the subject, which he illustrates by an account of six cases in which he had operated thus—

1. To open the bowel from the perineum is the best method of treating cases of imperforate anus.

2. It should be done, whenever it is possible, by Amussat's method, which makes the operation both less serious at the time, and more certain in its result.

3. The operation undoubtedly presents difficulties both in discovering the cul-de-sac of intestine, and in keeping the aperture open and in connexion with the external wound.

4. Partial removal of the coccyx, to the amount of about half an inch, diminishes all these difficulties.

5. It permits, without any marked bad results, the area of the operation to be much enlarged, the rectum to be reached at a higher level, the bowel to be fixed to the skin without its being torn or stretched, or without the use of too much force, and without the risk of a dangerous amount of separation of the bowel from the parts above and in front of it.

6. It saves a blind groping in the depth of the pelvis, and diminishes the risk of an accidental wound of the peritoneal cul-de-sac and the urinary organs.

7. It, to a large extent, abridges the time occupied in the operation.

8. Easy to execute, it does not appear to have in itself any additional danger whatever.

9. The resection of the coccyx is not always necessary; if, after some attempts, one does not find the bowel by the skin wound, or if the bowel when found does not seem to come down easily, then it should be done without hesitation.

10. If, after this resection, the rectum cannot be found, the perineal operation may be given up, and the operation in the iliac or lumbar region must be attempted.

11. The perineal operation, even successfully performed, may leave as its result a peculiar form of ano-urethral fistula, due to the continuance of an embryonic condition.—*Gazette des Hôpitaux*, Nos. 87 and 90, 1873.

Part Fifth.

MEDICAL NEWS.

CENTENARY OF THE ÆSCULAPIAN.¹—(*Specially reported for the Edinburgh Medical Journal.*)—On 14th November, the centenary of the Æsculapian was celebrated by a dinner in the Douglas Hotel. The following were the guests:—The Lord Provost, the Lord Justice-General, the Lord Justice-Clerk, Sir William Gibson-Craig, Bart.; Lord Ardmillan, Sir Robert Christison, Bart., M.D.; Rev. Sir H. W. Moncreiff, Bart.; Sir George Harvey, P.R.S.A.; the Dean of Faculty, Q.C., M.P.; the Solicitor-General, Q.C.; Dr Paterson, P.R.C.P.E.; Col. Sprout, 91st Highlanders; Deputy Surgeon-General Elliot, Surgeon-Major Leitch, Rev. Dr Nicholson, Bishop Cotterill, Professor Allen Thomson, Glasgow; Professor Gairdner, Glasgow; Professor Leishman, Glasgow; Dr Fleming, Glasgow; Dr Watson Wemyss of Denbrae; Mr James Hope, D.K.S.; Mr William Walker, F.R.C.S.; Dr George Balfour, F.R.C.P.; Dr John Brown, F.R.C.P.; Dr Smith, F.R.C.S.; Mr Peacock, W.S.; Dr Somerville, F.R.C.P.; Dr Tuke, F.R.C.P.; Mr Thomas Annandale, F.R.C.S.; Dr Joseph Bell, F.R.C.S.; Dr Craig MacLagan, F.R.C.P. The following members of the Æsculapian were present:—Dr Smith, Dr Simson, Dr Omond, Dr Malcolm, Dr Combe, Professor MacLagan, Dr Paterson, Professor Balfour, Benjamin Bell, Esq., Dr Andrew Wood, Dr Dunsmure, Dr Gillespie, Dr Watson, Dr Lowe, Dr Weir, Dr Haldane, Professor Sanders, Professor Turner, Dr John Duncan, Dr J. M. Duncan, and Professor Lister. Professor MacLagan occupied the chair; and Dr Andrew Wood and Dr Omond were the croupiers. Letters of apology were intimated from the Lord Advocate and Principal Sir Alexander Grant.

The *Chairman* proposed "The Queen," and the "Prince of Wales and Royal Family," toasts which were cordially responded to.

The *Chairman* then gave "The Navy, Army, and Army of Reserve." With regard to the Navy, the *Chairman* said, that if the ships of the Royal Navy could speak with any other voice than that of their guns, they might truly say, *Tempora mutantur, nos et mutamur in illis*. The change in a mere æsthetic aspect was for the worse, for anything uglier than some of the modern ships he

¹ As a general rule, meetings of a purely convivial nature do not constitute the kind of matter which ought to occupy the pages of a scientific journal; but the circumstances of this festival are somewhat peculiar. Probably the Æsculapian is unknown to many of our readers. We refer them for information to the speech of the *Chairman*, and commend the *spirit* of the Æsculapian to the consideration of the medical fraternity in every place where there are enough of Doctors to form a medical coterie.

had never seen; they were, however, fitted for the exigencies of modern warfare, and it was satisfactory to know that the men who manned them were as ready as ever to maintain the naval supremacy of Britain.

With regard to the Army, whose deeds he would not stop to eulogize, he had to express his especial satisfaction, in having the good fortune to couple the toast with the health of Lieutenant-Colonel Sprot of the 91st Highlanders, in which regiment the old Æsculapian, whose name he bore, had spent the first years of a long professional life. He had personally to thank Colonel Sprot for having recently marched his splendid regiment out of their way to show them to the aged widow of their former medical officer, who, as a young married woman, had joined the 91st in 1811. This kind act showed that in Colonel Sprot the gallantry of the soldier was combined with the courtesy of the chivalrous gentleman. Most fortunately, at this time, Great Britain was not involved in any of those gigantic wars at which the world stood aghast; but it was impossible for them not to have their feelings strongly in sympathy with the gallant band who were at this time engaged in a campaign in Africa, where there was more danger to be met than glory to be won. It had been remarked that the Ashantee war was to be fought as much by the medical as by the combatant officers. All the best modern appliances of medical science had been put in requisition to protect the health of our troops. Military hygiene was there fairly on its trial; if it came out with success, it would be a great triumph for that science in which the Æsculapians were most specially interested; but, whether or not, he hoped that the authorities at headquarters would remember the value of the Medical Department of the army, and would see that they received that full consideration to which they were entitled, and which the universities and medical corporations of the country had always been most anxious to secure for them. He begged to conjoin the name of Deputy Surgeon-General Elliot and Surgeon-Major Leitch with this branch of his toast.

In coupling the name of Sir Robert Christison with the Army of Reserve, he commended the spirit in which Sir Robert had, by his example, stimulated the students of the University to take part in the Volunteer movement; and at the last inspection of the Queen's Edinburgh Rifle Volunteer Brigade, he had seen no one march past "at the double" with more elastic step, than the revered and plucky captain of No. IV. Company.

Dr M'Bain, R.N., Lieut.-Col. Sprot, and Sir Robert Christison severally returned thanks.

The *Chairman* then gave "The Houses of Parliament," to which toast Mr E. Gordon, M.P., Dean of Faculty, responded.

The *Chairman* then gave "The Lord Provost and Magistrates;" and in referring to public questions, he said he was afraid the Æsculapians would not be able to give the Lord Provost much

assistance on the water question, for he thought they were more disposed to go to the banks of the Rhine and the Garonne for their beverages than to the Moorfoot or the Pentlands. (Laughter.)

The toast having been drunk, the Lord Provost, after acknowledging the courtesy of the meeting, said he had gone into the Town Council with the purpose of endeavouring to make municipal government respectable; and it was high time to do so. (Laughter and applause.) He might not have been able to do much; but he thought, upon the whole, on rigid examination of himself, he had been able to do a little. The reason why municipal government was not what it ought to be, did not rest with the Lord Provost and Magistrates and Town Councillors,—it rested with the Æsculapians and gentlemen of Edinburgh who took so little interest in civic affairs. If they came forward as they ought to do, it would be a very different thing. He thought he had succeeded at last in raising them to a certain extent from their habitual ease and apathy; for they had come forward well to support the Council in the plebiscite which he had ordered to be taken. They had restored peace and good-will and good-fellowship to the city, and he thought he was able to see a pleasant prospect before him, very much as Petruchio said to Katherine—"Ay, marry, I see peace and happiness, and good-will, and everything before me; and all our difficulties are at an end." (Applause.) He ventured to say, that if they could only get the gentlemen of Edinburgh, generally speaking, to take a little more interest in Council matters, which he thought they were beginning to do, they would soon restore municipal government to the position it ought to occupy. (Applause.)

In proposing the "Guests of the Æsculapians," the Chairman said—Æsculapians, the toast that I have now to propose to you is to be regarded as the toast of the evening. It is to the health of our guests who have done us the honour to come here to celebrate the hundredth anniversary of the Æsculapian. We are met here as social men to enjoy social intercourse. We have surrounded our table with those who may be regarded as the representative men of the Scottish metropolis, and from their presence we may infer that the community which they represent is not out of sympathy with the aims and objects of the Æsculapian. To enjoy social festivity, however, something more is required than merely that the host should be hospitable, and the guest willing to be entertained. There must always be some reserve, possibly stiffness, where they do not know each other thoroughly, and are not acquainted with each other's personal and domestic history. We know our guests—they may know us individually—but I suspect many of them do not know us in our corporate capacity, and I therefore may be permitted to give them some account of the Æsculapian. I am enabled to do this by having had access to two most interesting volumes of Minutes, which, under an interchange of very formal documents, are deposited in the library of the Royal College of Physicians of Edinburgh. The

Minute bearing date 2d April 1773, without any preamble, states briefly, and in a businesslike style, the origin of the Æsculapian.

"The Æsculapian Club was conceived in a happy moment, and in due time brought forth. Drs Duncan and Hamilton, Messrs Hay, Bell, M'Lure, and Dewar assisted at the birth on the first Friday of April 1773. They also stood godfathers at the christening. The various rites and ceremonies usual on these occasions, such as eating a good supper, etc., were concluded with a bumper recom-mendatory of the Club to the peculiar care and patronage of Apollo" (this is intelligible, because Apollo was the father of Æsculapius), "Bacchus" (this is intelligible, because it shows that the Æsculapians did not intend to sit dry-lipped), "and Venus."

This rather formidable-looking dedication is explained by the Minutes, which show that it means that they were to meet upon Friday, *Die Veneris*.

They seem at first to have contemplated doing something towards the promotion of medical science, for in 1777 they proposed to institute a gold medal for the best essay on some scientific subject. They appear, however, to have very soon discovered that they had quite enough to do in taking charge of the social concerns of the Æsculapian, for they abandoned their idea of the prize medal in 1778. There appears to have been some diversity of opinion upon this subject, for Dr Duncan wanted to resign the office of Secretary. He did not do so, however; and we learn from the subsequent minutes that he continued to act as Secretary until 1st December 1827. It is probable, I think, that this abandonment of the plan of giving a medal for the promotion of science led to the formation, in 1782, of another Society—the Harveian—which is now approaching its centenary, and which continues still to give from time to time a prize, for an essay upon some experimental subject.

Along with this Minute we have the list of original members of the Society. They are as follows:—Mr William Chalmer; Mr Andrew Wood;—and here it is interesting to note how Æsculapianism runs in families, for we have his grandson in the person of my friend Dr Andrew Wood, to-night discharging the duties of croupier at our festival; Dr James Lind; Dr James Hamilton. Dr Hamilton is well known to us by his *sobriquet* of "Cocky" Hamilton, as he continued, up to a period within the recollection of many of us, to wear the *chapeau à trois cornes*, the Court cut coat, knee-breeches and silk stockings, with his chubby hands exposed to the cold in the sharpest weather. Mr Thomas Hay, who, my Lord Provost, afterwards became City Chamberlain; Mr Benjamin Bell. Here we have another example of Æsculapianism running in the blood; for we not only have the pleasure of seeing his namesake and grandson among us to-night as a respected Æsculapian, but we have the pleasure of receiving among our guests his great-grandson in the person of my friend Dr Joseph Bell. Mr Forrest Dewar; Mr Hamilton M'Lure; Mr Colin Lauder; Mr John Balfour, who

was, I rather think, a member of the old family of Balfours of Pilrig, and if so, was at least a collateral ancestor of our guest, Dr G. W. Balfour, the editor of the *Edinburgh Medical Journal*. Dr Nathan Spens: his face is known to most of us from a very admirable portrait of him in his capacity of an archer, by Raeburn, in the Hall of the Queen's Body-Guard, and which is known to many more by the admirable engraving of it. Mr Alexander Wood: this gentleman is better known to all of us by his designation of "Long Sandy Wood." It is interesting to us to bear in mind that he is the great-great-grandfather of the sons of the Right Honourable and learned Lord Justice-General, who sits on my left hand. Mr Alexander Hamilton: this gentleman was afterwards Professor of Midwifery in the University of Edinburgh, and was the father of my former teacher, the late Professor Hamilton. Dr James Hay: he afterwards became Sir James Hay of King's Meadows, and was the ancestor of the present Baronets of that title. Dr John Hope, who was Professor of Botany in the University of Edinburgh, and father of my late instructor, Dr Thomas Charles Hope, Professor of Chemistry. Mr Thomas Wood, who was a brother of "Long Sandy;" and, lastly, Dr Andrew Duncan. Dr Duncan many of us are old enough to remember. He retained his vigour and energy up to a very late period of his life, continuing to ascend Arthur's Seat upon May morning until he was nearly, if not actually, an octogenarian, cultivating his garden out in St Leonard's with the motto *Hinc sanitas* upon the door; and many of us remember him going about the streets of Edinburgh as an old man, with, what my colleague the Professor of Botany would call, his specific character of an umbrella with a hooked ivory handle. Such were the original members of the Æsculapian. The remarkable characteristic of the Minutes is the constant recording of bets made among the members. These were always either in magnums of punch or magnums of claret, and the magnum of claret was also the maximum, for no higher bet was permitted to be taken. They were always even bets; on no occasion do I find odds given, except when in one instance Dr Monro bets two melons to one. It is clear that these bets were essentially an ingenious device by which the fluids necessary for the consumption of the party were provided by individuals. In fact, I find that in 1790 Drs Hamilton and Wardrop, without the formality of a bet, confess the loss of a magnum each *pro bono publico*. Everything and anything seems to have been enough to become the subject of a wager. Occasionally they indulged in a good-humoured way in discussions on politics. Thus, in 1782, we find a bet taken as to the chances of Lord North being out before the end of the session. Later in that same year, a bet is taken that Mr Fox is to be out in a year; and we find subsequently discussions with regard to a person who is called Mr William Pitt, Dr Duncan offering to give claret if he is Premier in five years,

Mr Wood offering the same if he is not, and Mr Benjamin Bell offering to give it for the national good whether Pitt is in or out. We thus see that this great surgeon, so distinguished by his surgical writings, was equally versed in Therapeutics and Hygiene, and saw the national importance of the Æsculapians getting their due supplies of claret. Passing events occasionally engaged them. Thus we have—of course again a bet of punch or claret—a wager that Lord Howe will have an engagement with the allied fleets before his return home. This was in October 1782, when Lord Howe sailed for the relief of Gibraltar, landed men, ammunition, and provisions, and offered battle to the allied fleets, which they refused, esteeming discretion the better part of valour. Among passing events we have one of a rather touching character. Dr Duncan bets that Miss Kitty Morrison shall be married in twelve months to a friend whose name he whispers into the ear of Dr Rutherford, the grandfather of our present Æsculapian, Dr Daniel Rutherford Haldane. I regret that I have not been able to find any record of what became of Miss Kitty Morrison, whether she became a wife and a happy mother of children, or whether she spent the remainder of her days in single blessedness. It has just occurred to me that it might be possible to get some interesting information upon this point by referring it to my honourable and learned friend Lord Ardmillan, who might possibly be able to make out whether Miss Kitty was a sister or other relative of Mary Morrison, who has been made immortal by Burns in one of his sweetest songs. Professional questions, which are with us now *vetitum nefas*, occasionally make their appearance. Thus in 1790 some claret is staked upon this issue, that Mr David Stewart's child, when it recovered from hydrocephalus, did not get any mercury; and for the settlement of this question reference is made to the books of Mr Alexander Wood; and in another minute we find Mr Thomas Hay betting, that he will produce certain very disagreeable effects within two hours upon any individual, by administering to him a drachm of ipecacuanha infused for twenty-four hours in one ounce of vinegar. Philology is not neglected. Thus we have a discussion upon the word "Bartisan," but all the information that we have with regard to it is that Dr Hay produced a learned Greek work bearing on the subject. Ethics are also discussed, Mr Alexander Wood staking a magnum of punch upon the thesis that a man most attentive to his own interests is the most generous man. How this was settled I do not know; but probably by seeing that his own interests in the matter of liquor were duly attended to, he may have found himself to be most generous to his brother Æsculapians. Literature also makes its appearance, Mr M'Lure referring to the votes of the company the question, if the poet Burns is "superlative in his degree." If Mr M'Lure, who seems to have supported the negative in this question, were alive now, I think he would find himself in minority. They do not hesitate even to deal with questions of

law. Thus we find—of course again under the sanction of a wager of claret—the statement made, that a card, though holograph, is not strictly binding in law; and the settlement of this question is referred to the Dean of Faculty. One grave omission I observe in the Minutes with regard to this, which is, that the Dean of Faculty does not appear to have been asked to dinner on the occasion of his delivering his opinion on the matter. I hope, however, that my learned friend, the present Dean, will understand that we have no intention to endeavour to extract from him any opinion upon this intricate subject, otherwise than in the regular way of memorial by the hands of an agent. There is a variety of subjects of wager which I can classify in no other way except as miscellaneous, of which I give only one example. Mr Lauder pays a pint of claret for having a bigger belly than Mr Hay. This seems to have been a sort of fine inflicted upon him for being *totus teres atque rotundus*. They do not scruple even to discuss questions of theology, for I find—of course again as the subject of a bet—a discussion as to the question, whether or not the whole tendency of Mr Ritchie's sermon to the Volunteers, on the 1st January 1796, is to support the doctrine of predestination, and this is referred to Dr Moodie and other divines. The great subject of bets, however, in almost every minute, is their matches at golf. This seems to have been the great athletic exercise of the Æsculapians, sometimes in twosome, sometimes in foursome matches; and we have records of not merely who lost or won, but occasionally of the number of strokes played in each match, four rounds of Leith Links being in general the extent of their golf competitions. And in reference to this I have to note a curious feature in these old Æsculapians. They had from time to time what they call a Gynnastic Convention, when they met to compete for prizes in various athletic exercises, chiefly golf, bowls, and swimming. A number of them were members of the Royal Company of Archers, and were prize-winners there; and I am happy to think that the practice of drawing the long-bow in the field is not extinct among Æsculapians, for my brother Æsculapian, Dr Dunsmure, now present, is at this moment the holder of the much-coveted prize of the silver arrow given by the city of Edinburgh. The records of these gymnastic feats consist in the medals appended to the three silver cups which are before me on the table. Many of these are very interesting, merely as recording the date upon which well-known individuals joined in these gymnastic competitions. Some of them are in English, many of them in Latin. I quote only one example of the latter. It bears this inscription, that *Gulielmus Cheyne, inter natandi studioso e conspectu quidem telluris pontum pectore pulsans, victor renavit*. This idea of a doctor swimming out of sight of land, like a sea-going steamer, appears to us rather a strong statement in these modern days; but what the medal does not tell, tradition explains, and we are told that on the occasion in question, when the party had met for their

swimming competition, there was, *horresco referens*, a decided "easterly haar;" that Dr Cheyne was the only man who ventured into the water; that he soon lost sight both of land and of his companions, and, speedily returning, claimed and obtained the prize. They seem not to have been contented, however, merely with their gymnastic conventions and their social meetings, for once or twice they gave evening parties for the benefit of their wives and families. Thus, in 1791, we have it stated in the Minutes that there was to be a "Grand Corruscation" or oyster-party, and we have not only the fact recorded, but we have the names of the gentlemen and their partners who formed the assemblage. These are mere samples of what is contained in the *Æsculapian Minutes*, and I have confined my remarks to the earlier periods, not wishing to occupy your time by tracing them on to the period of our own existence.

I have looked back at the history of the *Æsculapian* from its beginning. May I be permitted to look forward? I am led to indulge in a little bit of fancy from seeing present among our guests the great-grandson of one of the original founders—Dr Joseph Bell; and I am led to think of some little Bells in a house in the west end of the town, who I hope at this hour of the evening are sleeping the sleep of innocence in their respective cribs, but who probably a few hours ago were chiming out peals of merry infantile laughter in a certain nursery in Melville Street, and it seems to me possible that some one of these,—still very young

*Porrigenas teneras manus,
Dulce rideat ad patrem,
Semihiente labello—*

may in 1973, looking back to a long and honourable line of *Æsculapian* ancestors, be called upon to preside at the second centenary of the *Æsculapians* when the gravestone of your present chairman is moss-green and weather-beaten. The *Æsculapian* may perish—like all other human institutions, *absit omen*, it may decay; but though it perishes, its principles will remain, for they are immortal. Viewed apart from the social entertainments, which are merely accessories, the principles of the *Æsculapian* are neither more nor less than these: to promote in our own circle, and, so far as our example goes, beyond our circle, peace on earth, by cultivating good-will between man and man. It may be said that, after all, our meeting is merely a piece of epicureanism, and that we are merely men engaged in an arduous profession, who like to meet together from time to time, and enjoy our meat and wine and social chat, to exclude from our minds those cares and cares which are inseparable from every arduous profession. I do not mean to say that even upon this very low ground the *Æsculapian* is not defensible. Men who are engaged in any anxious profession, be they doctors or lawyers, or what not, are fairly entitled to refresh their minds from time to time by changing the currents of their thoughts, just as they are entitled, at

their holiday season, to change the air that they breathe, and so refresh their bodies. But Æsculapianism means something better. If we did not know it from our own experience, we have it told us by the highest of all teachers, that it "must needs be that offences come;" and the object of Æsculapianism is not merely to prevent offences from growing, but by clearing the ground properly to prevent them from ever germinating; and if I wished to express truly the basis upon which Æsculapianism rests, I would be content to rest it upon this one short but pregnant sentence of the great apostle to the Hebrews, "Let brotherly love continue."

Æsculapians, these remarks, addressed to the company generally, have been only introductory to the toast, which I now more especially address to you, of "Our Guests." We drink to them collectively. We know them individually, and I ask you, on looking round at those who surround our table, whether it was a rash statement that I made in the outset of these remarks, that we had with us to-night the representative men of the Scottish metropolis. We have present among us divines whom we always gladly welcome to any of our social meetings; we have the brave defenders of our country; we have men learned in science belonging to this and other Universities; we have the majesty of the law represented in those potent, grave, and reverend seigniors, whom we are always so glad to meet in their social capacity, and whom we all earnestly eschew in the discharge of their official functions; we have art represented by the genial President of the Royal Scottish Academy; and we have literature with us, not merely medical literature, in the person of the Editor of the *Edinburgh Medical Journal*, but general literature as represented by Dr John Brown, or rather I should say by Rab among his Friends. We should be glad to hear one and all of these individuals in replying to this toast; but the timepiece behind me warns me that the hours are fast passing away, and that I have consumed an inordinate amount of their constituent minutes. I shall therefore connect this toast with the health of one who is well entitled to be the representative of the representatives. To you it is unnecessary that I should say anything with regard to the Lord Justice-General, for you all know him as well as I do. You have all marked his brilliant career at the bar, his important and useful work in Parliament, the benefit of which I, in common with all other Professors of Scottish Universities, am now enjoying. You have marked the dignity and ability with which he discharges his high official duties, and I ask you, therefore, to connect his name with this toast. Rise, then, Æsculapians, show that our hearts are warm and our lungs sound, by a hearty cheer to The Guests of the Æsculapian. *Nunc est bibendum.*

After the toasts had been heartily honoured by the Æsculapians, the Lord Justice-General returned thanks, remarking that he had hoped to hear from the Chairman some history of Æsculapius and his wonderful doings; but that from what he had learned, he had

almost begun to think that he was mistaken about the name, and that he was rather amongst the disciples of Epicurus. (Laughter.)

The remaining toasts were, "The Universities of Scotland," proposed by the Solicitor-General, and responded to by Dr Allen Thomson; "The Chairman," proposed by the Lord Justice-Clerk; "Clergy of all Denominations," proposed by Dr A. Wood, and acknowledged by the Rev. Dr Nicholson; and "The Colleges of Physicians and Surgeons of Edinburgh, and the Faculty of Physicians and Surgeons of Glasgow," proposed by the Dean of Faculty, and responded to by Dr Paterson, Dr Simson, and Dr Fleming.

MODIFICATION OF THE OPERATION FOR HARE-LIP. By Sir WILLIAM FERGUSSON.—On two successive Saturdays, Sir William Fergusson has recently demonstrated to the students of King's College a novel modification of the ordinary operation for hare-lip. The cases in which he carried out this plan were of the usual type, the fissure being, as in the majority of instances, on the left side; and in both it was considered advisable to take away the inter-maxillary bone. This adds to the success of the operation, not only by removing an occasional obstacle to primary union, but because the teeth, which are subsequently developed from the projecting knob, are worse than useless, by reason of their deficient development and faulty position. Instead, however, of removing the portion of bone readily with the knife or bone forceps, it had occurred to Sir William that it would be much better to operate subcutaneously, so to speak, by stripping off and retaining the mucous membrance; and accordingly this was done with perfect success, the bone shelling out readily from its investment. Not only will this procedure greatly accelerate the subsequent process of healing, but the advantage is obvious of retaining a thick and firm mucous surface in preference to the more artificial substitute of cicatricial tissue.

HOW TO KNOW WHEN AN EGG IS FRESH.—Eggs are fresh during one or two days in summer, from one to six days in winter. The shell being porous, the fluid of the interior evaporates, and the shell becomes not so full. When an egg is stale, the yellow falls to one end, as may be seen by holding it up to a candle or to the sun; and if one shakes it, a slight shock is felt, which is not felt if the egg is fresh.

To tell the age of eggs, dissolve about four ounces of table-salt in forty ounces of water; and when it is quite melted, plunge the egg into it. If it is only one day old, it will touch the bottom of the vessel; if two days old, it will sink, but not to the bottom; if three, it will float near the top. If five days or more, it will appear at the surface, and project more and more according to its age.—*Revue de Therapeutique Medico-Chirurgicale*, 1873, No. 20.

PHOSPHORUS IN NEURALGIA.—My custom is to commence with

one-hundredth of a grain, and gradually increase this by one-fiftieth of a grain at a time, until, if necessary, one-tenth of a grain is taken with each dose. Beyond this quantity I do not go; as I think that, if the remedy be of use, relief will be attained by this dose equally with a larger. After trying several preparations, I now use a formula which Mr Potts, dispenser to the Manchesier Royal Infirmary, hit upon, and which seems to answer every purpose, in being tasteless, transparent, and readily prepared. He dissolves ten grains of phosphorus in two ounces of ether, agitating the solution from time to time; and of this solution, one minim (containing one-hundredth of a grain) is administered in an ounce of water with half a drachm of glycerine. The glycerine suspends the phosphorus so perfectly that a transparent mixture is the result. The addition of a little bitter infusion entirely removes any *suspçon* of lucifer-matches which may hover about the medicine.—S. MESSENGER BRADLEY, Manchester.—*British Medical Journal*.

PROGRAMME D'UN CONCOURS OUVERT PAR LA SOCIÉTÉ SUISSE POUR LA SANCTIFICATION DU DIMANCHE.—*Le repos du Dimanche au point de vue Hygiénique*.—Un concours est ouvert pour une étude sur l'importance hygiénique du repos du Dimanche au point de vue de tous, et en particulier de ceux qui sont voués aux professions laborieuses.

Indiquer sous une forme aussi populaire que possible:—1o. Les effets favorables de ce repos pour l'individu à ses différentes âges, et son influence tant sur la famille que sur la nation. 2o. Les maladies qu'un travail continu provoque ou aggrave chez ceux que leurs occupations privent du repos hebdomadaire, par exemple les ouvriers des métiers assujettissants, ceux de certains ateliers et usines, les employés des chemins de fer, des télégraphes, des postes, etc. 3o. Les applications pratiques qui découlent des considérations et des faits qui auront été exposés.

Une somme de 1200 fr., en un seul prix ou répartie en accessits, est mise dans ce but à la disposition du Jury. Le terme du Concours est fixé au 30 Septembre 1874. Les mémoires seront en langue française ou allemande, ou bien accompagnés d'une traduction, et devront être lisiblement écrits. Ils porteront une devise, qui sera répétée sur un pli cacheté contenant le nom et l'adresse de l'auteur.—Ils seront adressés au plus tard le 30 Septembre 1874 au Président de la Société suisse pour la Sanctification du Dimanche, Place de Champel, 497, Tranchées de Plainpalais, Genève. Les mémoires couronnés deviendront la propriété de la Société qui ouvre le Concours.

Le Jury sera composé de MM. les docteurs:—Bourgeois, Berne, *Président du Jury*; Bonnard, Lausanne; Dunant, Genève; Cornaz, Neuchâtel; Ernst, professeur, Zurich; Favarger, Neuchâtel; Hægler, Bâle; Koecher, professeur, Berne; Lombard, Senior, Genève; de Laharpe, Phil., Lausanne; de Mandach, Schaffhor

OBITUARY.

SIR HENRY HOLLAND, Bart., M.D., F.R.S., D.C.L.—By the death of this eminent man, which took place on his eighty-fifth birthday, at his house in Brook Street, London, one of those connecting links, which we find uniting the generations, has been broken; and almost the last of the old type of London physicians has passed away. Until (in 1872) he published his "Recollections of Past Life," previously printed for private circulation, he was, to most of the profession out of his own set in London, rather an historical character than an actual living man. It was difficult to realize that Dr Holland of Icelandic fame, who had travelled with Queen Caroline in 1814, when she was Princess of Wales, who had attended nearly all the nobles, premiers, and royal personages of the first half of the century, could still be alive. But to find, as was done from his book, that he was not only alive, but active as the youngest, that he was still, year after year, spending his holidays in the ends of the earth, revisiting Iceland after an interval of sixty years, devoting September to Jamaica, after an August in Scotland, to find that he was not old in aught but his years; that he was still bright and receptive, interested in the present and the future, brave, self-reliant, and hardy, more than most men independent of niceties of food or comforts of warm dress; no *laudator temporis acti* beyond its due; still less a *ensor castigatorem minorum*, for was not he himself also one of the men of the present?—we seemed to see in him a realization of the Ulysses of the English poet. Like him, Holland could not "rest from travel;" like him, he drank "life to the lees, and all times he enjoyed greatly;" and if more fortunate than the old Greek hero, Holland's life was so fenced round that he could hardly say, I "have suffered greatly;" he could fitly say—

"For always roaming with a hungry heart,
Much have I seen and known; cities of men
And manners, climates, councils, governments,
Myself not least, but honoured of them all;

I am a part of all that I have met;
Yet all experience is an arch where through
Gleams that untravelled world, whose margin fades
For ever and for ever when I move."

This is not the place for a biographical sketch, and even to name his travels would fill a volume. His life seemed to have been singularly fortunate, happy, and well rounded. He knew what he could do, what he would like to do, and he did it. In easy circumstances, he graduated in Edinburgh in 1811, when the society was brilliant, and he had access to the best. By his graduation thesis, he proved himself a born traveller, with an eye to notice and a pen to record what he saw. After five years spent in travel and study, partly with Princess Caroline of Wales, he settled in

London, where almost at once he achieved a brilliant success as physician to a *clientele* of rank and fashion; and as counsellor and friend, he seems to have been loved, trusted, and consulted by a large majority of the historical characters of his age. Placed in a position where he might, with ease, have amassed a large fortune, he was wise enough to refrain, to save time for writing, for watching the progress, if he did not actually propel the wheels, of science, for his beloved travels. To his wise self-restraint, and consequent leisure, he probably owed his long life and perfection of health. While not contributing much to purely professional literature, Sir Henry Holland wrote extensively on scientific subjects, climate, metaphysics, physiology, and the like; and he identified himself with the fortunes of the Royal Institution, for which he laboured, subscribed liberally, and was for nearly eight years its president. During the last few years of his life he gradually withdrew from the cares of practice, though still to many of his old patients he could not refuse his counsel.

His death was a fit ending of so well filled a life. After an excursion in Russia to the great fair of Nijni-Novgorod, he returned to London, but soon set off for Rome and Naples, returning by Paris, where he was present at Marshal Bazaine's trial on Friday, 24th October, and dined the same evening at the British Embassy in Paris, returning to London on Saturday, though then not well; he was still able to be out in his carriage on Sunday, when he drove to the door of a very old and valued friend, to whom he spoke in his carriage, feeling too weak to leave it, and died peacefully and painlessly on Monday the 27th.

Rarely does such a long, well-filled, and eventful life draw to a close so free from all evidence of decay of nature or failing health. Though his slight active frame was becoming bent, his step was as rapid and his eye as bright, his cheerfulness as perfect as ever. In speaking to him, you almost forgot, and he himself always failed to remind you, that he was an old man. He had none of the self-consciousness, little of the garrulity, of old age. No one could presume less on his eminence, his experience; no one was more interested in hearing of what was doing in science. Without being a profound diagnostic physician, or even a very fertile or sanguine therapist, his great sagacity, sound common sense, and immense experience, made Sir Henry Holland a most safe and reliable adviser. But to his tact, friendliness, and gentleness, his consummate knowledge of men and manners, he owed a position, as the friend and adviser of royalty, and of the heads of the State, such as hardly any one ever occupied before, and certainly no one occupies now. He could receive a secret, give advice on it, and *keep* it in a very safe and satisfactory way—almost too safely for posterity; for he tells us just enough in his "Recollections" to make us long for more.

His personal appearance was striking. Of middle height

slender frame, with compact, small head—broad, rather than high forehead, with a remarkably quick, attentive, and thoughtful eye, no one could fail to see the man of mark in Sir Henry Holland.

CORRESPONDENCE.

To the Editor of the Edinburgh Medical Journal.

DEAR SIR,—The letter of Dr Playfair, in your "Correspondence," recalls to my memory the anecdote, once related to me, of the heroic, but "successful" treatment of a case of cholera by a Yankee skipper, on board a Mississippi steamer.

It seems the patient was in a desperate condition. The doctor had given him up. "Eh! give him up: certain?" exclaimed the captain. "Then bring me a sheet, and all the mustard you find knocking about." A huge sinapism was improvised: the man stripped, and stretched on this novel couch, and, with the exception of his head, his entire body was closely swathed in this colossal cataplasm. Whether "*post hoc vel propter*," the patient recovered.—I am, Sir, your obedient servant,

WILLIAM MACFARLANE, M.D.

18 Argyll Road, Kensington, London, W.,
5th Oct. 1873.

PUBLICATIONS RECEIVED.

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| Julius Althaus, M.D.,—Treatise on Medical Electricity. London, 1873. | E. D. Mapother, M.D.,—The Prevention of Artisans' Diseases; and, Lessons from the Lives of Irish Surgeons. Dublin, 1873. |
| Joseph Bell, F.R.C.S.,—Manual of the Operations of Surgery. Edinburgh, 1874. | A. D. Rockwell, M.D., and Geo. M. Beard, M.D.,—Clinical Researches in Electro-Surgery. New York, 1873. |
| Edward Bellamy, F.R.C.S.,—The Student's Guide to Surgical Anatomy. London, 1873. | D. B. St John Roosa, M.D.,—Practical Treatise on Diseases of the Ear. New York, 1873. |
| W. Fairlie Clarke, F.R.C.S.,—Manual of the Practice of Surgery. London, 1874. | Balmanno Squire,—Photographic Clinique of the British Hospital for Diseases of the Skin, Nos. 2, 3, 4. London, 1873. |
| Georges Hayem,—Revue des Sciences Médicales, tome ii. No. 2. Paris, 1873. | Transactions of the Pathological Society of London, vol. 24. London, 1873. |
| A. Jacobi, M.D.,—Infant Diet. New York, 1873. | Th. de Valcourt, M.D.P.,—Sketch of Cannes and its Climate. London, 1873. |
| George Johnson, M.D.,—Lectures on Bright's Disease. London, 1873. | A. Dunbar Walker, M.D.,—Egypt as a Health Resort. London, 1873. |
| Furneaux Jordan, F.R.C.S.,—Surgical Inquiries. London, 1873. | |
| George Lawson, F.R.C.S.,—Diseases and Injuries of the Eye. London, 1874. | |



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